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ORIGINAL ESSAYS.

An Essay on the Immateriality of Mind. By HENRY T. FARMER, M. D. Member of the Historical Society of New-York, the Medico-Chirurgical Society of New-York, and of the Medical Society of Charleston, S. C.

" There are more things in heaven and earth, Horatio,
" Than are dreamt of in your philosophy."

* * * * *

" We are such things as dreams are made of,
" And our little life is rounded by a sleep."

SHAKESPEARE.

IT is to be lamented that philosophy, which should be governed by truth and supported by wisdom, is too frequently shackled by some favourite hypothesis, or made subservient to some novel speculation. We consequently find her disciples determined to support their own opinions, rather than, to establish facts or detect inconsistency. Hence they desert the path of utility for some unexplored labyrinth beyond the limits of human evidence, and without the pale of probability. While they endeavour to explain some things in their nature inexplicable, they deny the existence of others, merely because they are unable to discern or understand them.

So long as philosophy is built upon the substantial basis of experiment, deductions are correctly formed; but, otherwise, change succeeds change, novelty succeeds novelty, materials are selected from the clouds of conjecture, and the wiles of ingenuity, like the shades in a kaleidoscope, glitter without value, and change without utility.

But there is another fatality which pervades them, namely, a spirit of hostile opposition, neither friendly to the acquirement of liberal science, nor in unison with unbiassed research; hence, not satisfied with erecting a system of their own, they endeavour to demolish the fabrics of those who preceded them, like those barbarians who defaced the beautiful temples of antiquity, in order to embellish some unhallowed shrine. Such hostile opposition has, I fear, too often characterised the disciples of the Healing Art, nay, it has been carried so far, that even conviction was found inadequate to stem its current: thus, when Cullen assailed the system of Boerhaave, he was bent upon absolute extermination. Instead of erasing merely the obvious traces of error, he determined, like the foe of Carthage, to leave not a single pillar standing; though, if I may be allowed to extend the simile, he was sometimes obliged to rest like Marius on its ruins. And here, I must be pardoned for remarking that the total rejection of the humoral pathology was a dark æra in the annals of medicine.

Where shall we seek for the boasted liberality of science? It may be found at the tombs of Harvey, Sydenham and Haller, where renown is heaped upon the bones of men, who during life were assaulted by the rudest clamours of ignorance, and wounded by the poisoned shafts of envy. *Then* no kindred hand plucked a wreath from the forest to crown them, but *now*—each honest votary of undisguised philosophy, casts a flower upon their graves. The fate of such men illustrates the correctness of Sterne's beautiful remark,

“ death opens the gate of fame, and shuts the gate of envy after it.”

But, shall we wonder at the spirit of repulsive contradiction existing between man and his fellow being, when we find some sceptics so daring as to dispute the edicts of omnipotence, and cavil at divine revelation ; when, we behold philosophy like some aspiring fiend decorating the shrine of infidelity, and asserting the materiality of that spirit which was doomed to be immortal. It is with sentiments of regret that the name of Doctor Priestley should be adduced in support of such an hypothesis ; how that reverend philosopher could reconcile his opinions with such as he was obliged to promulgate from the pulpit, is matter of considerable perplexity. He certainly could not place implicit belief in the soul's immortality, if he was sincere in his material doctrine ; and if he announced the word of God, with the lips of an unbeliever, he deserves at least our pity, if not our contempt. But, says Sophistry, does it follow that such as believe the soul, or mind to be material, must consequently doubt its immortality ? “ Is it probable, (says Rush) that a wise and good being, whose means and ends are exactly suited to each other, in such parts of his works as we are able to comprehend, will finally waste and throw away the beautiful apparatus he has given us for the enjoyment of corporeal and mental pleasures ? ” That the Supreme Being does allow this costly and beautiful apparatus to waste, must be evident to all who have noticed its gradual decay, during prolonged existence, and its sudden decomposition after death. And it is *more* than “ probable,” it is certain, unless the sacred legends of our religion are fabulous ; that the body is thrown away, for we are expressly told that “ flesh and blood cannot inherit the kingdom of heaven,” and that “ we shall be *changed* in a moment, in the twinkling of an eye at the last trump.” But mark the tacit acknowledgment of mental supremacy, from the hand of the same person

who asked the above question. "The pleasures of *sensation* are of a limited nature, as to their *degree*; no ingenuity has ever been able to raise them so high as to perfectly satisfy the mind." Now does it appear likely that the properties of "this beautiful apparatus" which are so inferior to the mind on earth, should after losing that beauty, be quite sufficient for the superlative happiness of the pure and eternal spirit in heaven? Alas philosophy! thy powers are dazzled and bewildered, but even when thou hast lost thyself, like some benighted pilgrim who mistakes a mere exhalation for some friendly light, still art thou too proud to call FAITH to thy assistance and follow her to the altar of simplicity and truth.

I now propose to take a view of some of Doctor Priestley's arguments in favour of the "*materiality of mind*," and here it may be necessary to premise, that these arguments are predicated on assertions, and supported by conclusions deduced from such assertions. Dr. Priestley contends, first, that the mind is material; and secondly, that the brain is the mind; for, says, he "there is *no* instance of any man retaining the faculty of thinking when the brain is destroyed—moreover as the faculty of thinking in general ripens and comes to maturity with the body, it is also observed to decay with it; and if in some cases the mental faculties continue vigorous when the body in general is enfeebled, it is evidently because in those particular cases the brain is not *much* affected by the general cause of weakness."

These are Priestley's assertions to which authentic testimony shall be opposed.

"The history of dissections (says Crichton) *proves* that every part of the brain may be morbidly altered from its natural state and yet *all* the faculties of the mind remain entire. The writings of the learned, industrious, and modest Morgagni, and those of Bonetus and Haller, contain many cases of this kind. Portions of the brain have been forcibly

detached by cutting instruments ; great excavations have been formed in it by abscesses ; schirrous or scrophulous tumours *two inches* in length, have been found in it ; fungous tumours have arisen from its surface ; all its arteries have been ossified ; its coats have been variously diseased ; the interior part of the cerebrum and cerebellum ; the basis cerebri, the pituitary gland, the *pineal* gland, the plexus choroïdes, have all been found exhibiting morbid changes of structure, in people who were in *full* possession of their internal senses."

"Is it to be taken for granted" (says Fordyce) "that the mind exists in the brain and governs the body, seated as on a throne, sending its messengers to other parts of its dominions ? A man, whose skull is fractured, is generally (in consequence of the injury done to the brain) delirious. An inflammation of the brain sometimes produces delirium (not always.) Injuries done to other parts of the body produce delirium, when there is not the smallest appearance on dissection of any injury done to the brain. The brain may be much altered, almost entirely converted into pus without delirium."

"Of this, instances have occurred in Doctor Hunter's dissecting room. In one of these, it was well known that there was no derangement of the mind during the inflammation and suppuration, but the patient was carried off by a disease in which the brain could hardly be conceived to have its material part at all disordered. Indeed, the author has in several cases seen it happen, that a delirious patient in fever, without *any* abatement of any of the other symptoms of the disease, has become perfectly sensible. In all these cases death followed shortly, as if *the mind had escaped from the disease of the body, before it left the body entirely.*" "Again, it is a doubt," continues Fordyce, "whether delirium may not arise in fever, as an affection of the mind only." But if it should be granted that delirium or mania were always followed by some

consequent injury of the brain, our opponents would derive little support from such concession. Mania (at least in its acute state) is mental derangement, not mental deprivation.

The quantity of mind remains the same, but like the chaos of philosophers its disposition is void of harmony. In this state, mind has frequently developed its loftiest powers, like some mysterious orb beyond the limits of attraction, it eliminates during such ungoverned anarchy beams of splendour more dazzling, and more magnificent than the radiance of regular stars. But in the chronic state (or fatuity) we might for a moment suppose that the mind was totally estranged from the body, and had left the idiot possessed of less than brutal instinct, and little more than vegetable life. The outposts of mania make their first attack upon the heart, from which the brain like a kindred spirit raises the oppressive burthen, and shares it between imagination and memory.

If mind is material, why should it be confined to the human body, why should mind be less general, than excursive, why should it not be evident in a stone, active in a whirlwind, and apparent in the vegetable kingdom? Let rebellious scepticism reply. But here I must be pardoned for remarking that truth is seldom elicited from conflicting opinion or specious sophistry. The warrior fights not for justice, but renown, so the literary combatant tramples over wisdom to snatch a crown from ingenious subtilty. Many dissent from a belief in the immateriality of mind because they cannot account for it. Upon such grounds scepticism might obtain a host of supporters, since the most profound philosopher is able to account for very little of what he yields his assent to. He believes the sun to be the centre of the solar system, without being able to account for the derivation of his light; he believes in a beginning and an end; he can account for neither; he makes use of the word eternity, when it is beyond the utmost depth of his conception; he knows that the blood circulates, but can account neither for its first impulse

nor final cessation. Now I contend, that this providential mystery is one of the chief sources of intellectual happiness. Was it possible to trace the Deity through all his works ; to look through nature at a single glance ; and to unfold the scroll of futurity ; the active powers of mind would immediately become satiated and remain dormant : investigation would trace her world to its utmost limits, and sigh like Alexander, because she had nothing left to conquer. It is a *mysterious* combination of light and shade that renders the ruined abbey so beautiful at moonlight. The eye dwells with serenity upon such lineaments as are perceptible, while imagination peoples the shadowy aisles with a wizard court of her own, and bids past ages rise in review before her. Shall we not consider the moonlight and the abbey an epitome of mind and matter ? The moonlight is no part of the abbey, but merely the medium by which we are enabled to survey its slumbering magnificence. To *light*, we are indebted for most of what is termed information, still nothing so plainly develops the very limited boundaries allotted to the senses ; for though the sun shining with splendour *assists* our optics, still we cease to behold the congregated stars which are evident at night. We can *not* behold the sun without suffering considerable pain ; this should teach aspiring philosophy that *she* is too weak to look over the battlements of nature, seeing that she grows dizzy on the first steps that lead to the lowest tower.

It appears that an insurmountable difficulty presents itself in the following question ; indeed even the celebrated Crichton, is unable to meet it. What can act upon matter, *but* matter ? Now, both the Philosopher and simple Indian ; nay, persons of every age and clime, *have* believed and *do* believe in a first cause. Such first cause has been termed the Deity, Nature, Providence, the Divine Essence, or Great Spirit ; and I will venture to say that none of these persons ever did conceive the *Omnipresent* to be material. No, not

even the supporters of the very doctrine, that nothing can act upon matter, but matter. How then is matter acted upon and created by the Deity, by a Divine Essence, a Great Spirit? Man, we are told, was created after God's own image; but this conformation relates to mind, not to exterior form, else should the afflicted cripple despair, while the "*Apollo Belvidere*" of the day, though sunk in the lowest depth of profanation and voluptuousness, might boast of resembling his God.

We know that some of the *faculties* of mind become evident, through the aid of the sensorial *faculties* upon which certain impressions are made, and hence it is argued, that as nothing can act upon matter but matter, mind must be material. But I contend that *all* the faculties of mind are *not* subservient to sensorial impressions, or there could be no such state as absolute abstraction. In this state though all the senses be solicited; though music combines her magic with the landscapes of Arcadia; though nectar glows upon the lip, and the arm of beauty sustains the declining brow; the mind is secluded in some mysterious shade, where she is busy, looking back for memory, forward for hope, or beside her for despair.

If the senses were the only media by which impressions are conveyed, we should be compelled at *all* times, to notice *all* things in our immediate vicinity without voluntary inclination. Now, it is frequently very difficult to fix the attention on surrounding objects, even though we may desire to do so. No, the mind has powers superior to such as are merely necessary for material or corporeal affinity; powers which we are unable to analyze or comprehend. It has been asserted that at the hour of death, the mind frequently evinces unusual force and splendour. If this be the case, is it not probable that after her total escape from the dross that surrounds her, she may resemble her most beautiful power, imagination, and realize in the fields of *infinite* space

and *infinite* magnificence, all her past dreams of delight and extacy? Let it not be supposed that because I have said that the outposts of Mania make their first attack upon the heart, I therefore infer that the *heart* is the mind. He who insists on the immateriality of mind would betray a total ignorance of his subject, should he attempt to confine *mind* to any specific circumference or bounds. Neither do I suppose that the mind is let loose amidst the whole commonwealth of body, as some have contended, for if this was the case, a surgeon who amputates a limb would lop off a portion of mind also. Where then is the mind? It is a paradox, it is like the presence of the deity, *every where evident, but no where visible*.

Having adduced testimony to prove that the brain may be morbidly altered, and converted almost entirely into pus, without any consequent obliteration or derangement of intellect; and having attempted to defend the dignity of mind against that Agrarian principle which would debase it to a degrading level, I shall proceed to the consideration of Dr. Priestley's last assertion, to wit; "that the faculty of thinking after ripening and coming to maturity with the body, was generally observed to decay with it."

It is matter of regret that our philosopher did not state distinctly what he meant by maturity; if it was *manhood*, his arguments would hang upon a flimsy attenuated thread that every breath might sever. This is like confining mind to the scale of the celebrated and excentric Dr. John Brown. After toiling up to the summit of excitement, mind descends gradually step by step to the opposite extreme of debility, where she is soon after buried with the rest of the body. If mind depends on corporeal maturity, the giant Goliath must have possessed a whole continent of mind. In future we shall be able to estimate the genius, and intellectual powers of every man, merely by refering to the proportions of his frame and its relative maturity. Ajax has new honors

in store for him, and Pope must be blotted from the scroll of renown; Pope—who like the crook-backed king was sent into the world “scarce half made up,” whose body was never matured, who was the victim of disease and corporeal imbecility, who was in fact the nobody of poets. But badinage apart. The declension of age has been frequently noticed, but shall we not readily account for such declension? The love of novelty has no common share in eliciting the faculties of mind, now he who sojourns in the world for almost a century, may exclaim like the ancient king, that there is nothing new under the sun, and consequently, those forces are allowed to slumber in listless vacancy which were formerly active and persevering. But where employment has not suffered the mind to relax, where the torch of investigation has not been extinguished, where perseverance has bent as it were over the very brink of the grave, in this case I say, mental vigour has been accelerated by the assistance of wisdom and experience.

Every art, every science furnishes examples of the validity of this remark. Time did not impair the designs of West, nor did music mourn over the descending glories of Handel. Homer’s strings were not rusted by age, nor was Milton’s harp shattered by decay. Did the satire of Voltaire become blunted at fourscore? Or was Franklin deserted by wisdom on account of his grey hairs? Did the faculties of Bacon and Newton decline with the body’s maturity? Was Johnson despoiled? Was not Burke always “sublime and beautiful?” And, who shall deprive Chatham of the glories that shone like a halo around his silver locks? The long neck of Alexander did not warp his intellect; deformity did not bend the spirit of Philopæmon; nor epilepsy weaken the judgment of Cæsar. These illustrious examples resembled the ancient temples of Vesta, where the sacred flame was never extinguished, till the surrounding walls were laid in ruins. Old age, says Cicero, does not extinguish genius,

provided active study be not abandoned. Plato died whilst finishing a work in his eighty-first year; Socrates completed a work in his ninety-fifth year; and his teacher, Leontius Gorgius, was equal to all the ordinary employments of life, at the advanced age of one hundred and seven. Sir Isaac Newton, when near his eightieth year, resolved as the amusement for the evening, the celebrated problem of the trajectories, which Leibnitz supposed would puzzle the best mathematician of the age. The venerable Hippocrates died in full possession of those eminent intellectual endowments which gained him at an earlier period a crown of gold: and the celebrated Mr. Hey of Leeds, is a living example, that neither age nor disease disarm the mind, where no relaxation is superinduced, but where the enthusiastic ardour of youth is combined with the experience of old age. "The ancient governments were supported and maintained by old men. In order to be a magistrate of Lacedæmon, it was necessary to be far advanced in life." To conclude, wisdom (by the general consent of poets and painters) has been personified as aged and infirm.

Morbid sensibility more frequently attends genius, than corporeal maturity, since the former is consistent with divine justice, and is indeed, a balance of power. One Being is endowed with intellectual vigour, attended with debility and a melancholy temperament, another is possessed of vast muscular power and good health: the one seldom envies the other. If Ajax scorned the *imbecility* of Thersites—Thersites sneered at the *imbecility* of Ajax. They were both strong, and both weak, the relative distribution of such strength was exactly opposite. The mind of Fabius was more forcible than the arm of Marcellus.

Dr. Rush is another supporter of materialism: in fact, Rush and Priestley resemble each other in many respects. Priestley asserted the unity of God, Rush the unity of disease: they were both fond of Theory—Both at variance

with their respective contemporaries and both materialists. But the luxuriant fancy of Rush betrayed him in an unguarded moment, when the ice of his philosophy melted away. In his Inquiry into the proximate cause of the pleasures of the mind, he says, "they are the effects of impressions of a certain definite or moderate degree of force, accompanied by motions of a regular or harmonious motion in the brain and heart, and *communicated* by them to the *mind*." Now, if this is not a desertion of his doctrine, it at least proves that Rush neither conceived the brain nor heart to be the mind.

But does mental pleasure depend upon a regular or harmonious motion in the brain and heart? These pleasures are paradoxical, they are frequently opposite, they are expressed by smiles and by tears; by the hurried circulation of sudden joy; by the gentle stream of tranquillity. Perhaps it would be equally difficult to discover the proximate cause of mental pleasure, as to account for the origin of mind. With all the Doctor's ingenuity, I am as sceptical as Corporal Trim, who, after listening to a philosophical discussion concerning radical heat and radical moisture, decided that the former was burned brandy, and the latter ditch water.

Let it not be supposed that I endeavour to undervalue or depreciate the talents of Priestley and Rush. To the former, philosophy is indebted for some of her most valuable acquisitions and fairest ornaments: whilst Rush smoothed the acclivities of research, and covered the wilds of science with flowers. Perhaps no author possessed the happy talent of illustration in a more eminent degree. He consequently imparted instruction by a channel that may be said to resemble the foetal circulation, since it was sent by a short cut to the heart. If he had errors, let it be remembered that he was human; but when such errors are forgotten, his beauties which are abundant, will wear that bloom which defies the damps of time, and the shafts of malignity.

It appears to me, that there has always been the same specific quantity of pure gold in the mint of human genius, and that we only give it the current stamp or fashion of the day, without adding to its primitive weight. If some branches of art or science aspire like surgery and chemistry, others decline. We can propel a steam-boat, but we cannot divine how such immense blocks of stone were piled on ancient buildings, nor can we equal their cement. The deserted columns of Persepolis and Palmyra remind us of our comparative *nothingness*; in fact architecture is never herself, but when she appears in ancient habiliments. The ancient orators have never been surpassed, and Homer still wears the tributary laurel in despite of his numerous successors. *By the by*, we find this father of poetry supporting the immateriality of mind in the following lines.

'Tis true, 'tis *certain*; man though dead retains
Part of himself; the immortal mind remains:
The form subsists without the body's aid,
Ærial semblance, and an empty shade.

Let it be remembered that Homer scanned the Arcana of nature with the inspiration of genius united to the simplicity of childhood; he had no favourite hypothesis to support, no governing system to confound; his opinions were consequently unpolluted, "like the gentle rain from heaven, upon the place beneath." The mind of Homer was like sparks from a diamond of the first water; assisted by faith we may now behold them emancipated, and expanding into constellations of effulgent and eternal lustre.

From the tenor of these remarks it should seem that mind is not material, not a prisoner in the brain, since its faculties remain unimpaired when the brain is morbidly changed and almost converted into pus. That the faculty of thinking does not decline with the maturity of body, provided active study be not abandoned. And that so far from health being necessary to mental development or sustenance, its supe-

rior force has been associated with disease and evinced at the hour of death.

I shall conclude by soliciting the indulgence of the reader : the vital importance of my subject, demanded talents of a higher order than my humble resources could furnish. I have defended a tower, with a handful of reeds. The task must devolve upon some happier being, whose argument shall resemble a Corinthian pillar—his illustrations the beautiful Acanthus that winds about its capital.

On Amputation performed at the Joints. By THOMAS HUBBARD, M. D. Vice-President of the Connecticut Medical Society.

In a late number of the Medical Repository I observed a paper on amputation at the joints, by Dr. Mann. He states, that there is recorded in American practice, to his knowledge, but one case of joint operation, excepting of the shoulder at the scapular articulation. This was an amputation at the wrist joint performed by me. I have amputated at the wrist joint in two instances ; but have published no account of either. The facts were communicated in conversation, some years since, to Professor Smith, of New-haven, and Dr. Cogswell, of Hartford, in this State, and to other gentlemen of the faculty. It is probable that Dr. Mann, having heard of one of these operations, was led to infer, that I had published an account of it. About twelve years since I was called to a patient, a labouring man, who had his hand dreadfully lacerated by the sudden and unexpected bursting of a rock by gunpowder. The thumb, with some part of the carpus, was carried several rods from him by the force of the explosion.

It occurred to me, that a stump made by amputation at the wrist would be much more useful, than one left after an ope-

ration of the same kind performed above the joint. The enlargement of the lower ends of the radius and ulna, and the process of the latter, make it very convenient to retain a strap passed around a little above the ends of the bones, with a loop to attach any instrument of labour. In this case the radial and ulnar arteries were divided, and the integuments on the palmar side of the wrist, so as to make a flap of a tolerably convenient form. I made a flap on the dorsal side of the hand, and completed the division of the ligaments and tendons between the ends of the bones of the fore arm and carpus: after securing the arteries, I dressed the stump with adhesive plaster, &c. in the usual manner. The operation succeeded well, and the man was enabled to pursue his former occupation of blowing rocks. Having lost his left hand, he passed his drill through a loop in a strap as before mentioned, and thought he could perform that kind of labour as well as before. The stump, when healed, was remarkably free from tenderness. I have seen him strike it with considerable force against a board; it appeared no more tender than the knuckle joints of labouring men. Some years after another case occurred of a similar accident, occasioned by the bursting of a gun; I operated in the same manner, and with equal success. Dr. Cogswell observed to me, that since I had communicated to him this mode of operating, he had performed two similar operations, and with perfect success. Dr. Mann opened the lips of the wound and introduced dressings, after the operation, to facilitate the discharge, which he remarks might not have been necessary. I think that whenever an accumulation of pus or serum takes place under the flaps after such an operation, the occasional introduction of a probe, to separate gently the lips of the wound, is all that can be required. This would be attended with less irritation, and of course, the wound would heal more rapidly. It may be said, that I ought not to have been satisfied in my first operation, with making a flap of the lacerated

wound on the palmar side of the wrist; but that I should have made a new incision; I would observe, that the wound was nearly in the shape that I wished to form a flap, and there were no integuments to spare. I made use of such a flap in the case of an amputation at the shoulder joint, in which the arm was nearly torn off by machinery in a cotton mill. The joint was torn open, the scapular end of the clavicle separated from the scapula, &c. I made another flap to meet the one already made, and the operation succeeded completely. In amputations at the joints of the fingers or toes, I am not solicitous on what part of the joint I make the flap; provided I can save skin enough to cover the stump. In cases of mortification the effect of frost, the sound skin may be very irregular; but I seldom remove any of it. In penetrating wounds of the finger and toe joints, where inflammation and suppuration occur, producing great constitutional irritation, I have succeeded well by dissecting out the phalanx, and saving skin sufficient to cover the stump. When the finger, or toe, is half, or more separated by the original wound, and the symptoms above-mentioned occur, I barely carry my knife to the bottom of the wound and cut obliquely outwards, removing the bone and making the flap, at a single stroke. This practice may not seem correct; but it has invariably proved successful. The inflammation, with all its symptoms, has speedily abated, and the stump has healed kindly. Serous membranes become inflamed by wounds that do not heal by the first intention; but when their continuity is destroyed, by extirpating a large portion of them, the inflammation will frequently cease.

Such have been the effects of amputation after wounds at the joints of the fingers, and toes in my practice. In amputations at the wrist joint, I think it very important to remove all the bones of the carpus. I was once called upon to visit a case, where this direction was not attended to; but unfortunately the patient died before my arrival. This is the

only instance in which, any fatal, or even dangerous consequences have come to my knowledge, from this operation.

Of amputation at the elbow, I think very favourably, and feel under obligations to Dr. Mann for his communication of a case.

Amputation at the knee is performed, as a punishment, in the Turkish dominions, as appears by the statement of Captain Riley, in his narrative of a captivity among the Arabs. In such cases the operations are performed by a common butcher, and doubtless with success, as no mention is made of such punishments proving fatal. Surely the operation might be performed, with equal success, by a scientific surgeon.

Pomfret, Con. January 8, 1822.

Observations on Hydrophobia ; pointing out the means of detecting the existence of the Virus in the System, and of preventing its development, by destroying its Germ. A memoir read before the Medico-Physical Society of Moscow, on the 4th of October, 1820. BY MICHEL MAROCHETTI, Physician to the Hospital of Galitzin, and Member of the Society.

(Translated for the Medical Repository.)

Among the grievous accidents to which man is liable, the effects of the bite of a rabid animal are the most terrible ; and medicine has heretofore been wholly inadequate to afford any relief when once the poison is *absorbed*, and its effects on the system have become evident. That Hydrophobia is absolutely incurable, I shall not attempt to refute, but with a knowledge of the cause of the disease I do assert, that it is possible, by timely and proper aid, to prevent its development and retrieve the unhappy sufferer from the inevitable death that awaited him before the important discovery, which

I have now the honor of making known to the medical profession.

The seat of this disease has heretofore remained entirely unknown, and a great variety of medicaments have been employed, some of them doubtless *with success*, in persons not affected with the disease, and others as prophylactics in a philanthropic spirit of attempting something to ward off so great an evil. The important point to be gained, and which physicians have sought for in vain, is the cause of the disease—its direful effects are but too well known, and accordingly, I shall confine myself in this memoir to making known; 1st, what observation has taught me concerning this disease; 2nd, the origin of the discovery; and finally the different cases of the disease which I have been enabled to collect, as well as those which occurred under my own observation, and which were submitted to this new method of treatment. Having resided nearly eight years in the southern departments of Russia, where dogs greatly abound, and rabies canina is very prevalent, I had many sad occasions of seeing the victims of their ravages; and was consequently led to investigate the nature of hydrophobia, and to attempt every known method of relief. Surrounded by many persons who had been bitten by rabid dogs and wolves, no one could have had a better opportunity than myself of attentively observing the character and progress of this horrible disease.

A scrupulous observation has enabled me to draw the following conclusions :

1. That of many persons successively bitten by a rabid animal, the first bitten will have more violent hydrophobic symptoms than the second, the second than the third, and so on, up to the eighteenth or twentieth person, which last may be considered in little danger of having received the poison.
2. The hydrophobic virus is exclusively lodged in the mouth of the rabid animal, and as it takes a certain time for

it to accumulate, a bite in the interval will not be venomous ; which constitutes a second condition in which a rabid animal will not communicate the disease.

3. The hydrophobic virus does not, like *pestilential miasmata*, lose its intensity of action by being communicated from subject to subject, but always acts with more or less violence according to its quantity. Unfortunately its effects are always the same when once the disease is declared.

4. That the virus does not remain in the wound, but is taken up and deposited unaltered in a particular part of the system, where it acts as a most powerful astringent, and accumulating, inflames and blocks up the outlets by which nature attempts to expel it from the animal economy.

5. That there is only one way of preventing the development of hydrophobia in a person bitten by a rabid animal : I assert it confidently, and experience will confirm the assertion ; it is to evacuate the virus from its lodgment.

6. The two *sublingual glands* are situated under the tongue, one on each side, between the genio-glossi muscles and the inferior maxillary bone, and merely covered by the membranous lining of the mouth. Each of these glands have two or three ducts which terminate in the ducts of the sub-maxillary gland, and these latter open into the mouth on each side of the frænum of the tongue. After the bite of a rabid animal the virus is conveyed and deposited at the termination of these ducts, where it accumulates and is *temporarily* confined, forming one or two tumours of unequal size, which by means of a probe or sound, will be found to contain a fluid. This fluid experience will show to be the true hydrophobic virus, which it is necessary for the surgeon to evacuate in order to prevent the development of the disease.

7. The exact time at which these tumours become evident cannot be indicated, it is usually from the *third* to the *ninth* day after the accident. If the virus is not evacuated in *twenty-four* hours, it is *re-absorbed*, leaving no trace of its previ-

ous existence, and a double metastasis to the brain taking place, it produces the most frightful symptoms of hydrophobia, which invariably destroys the patient.—Examination of the body after death has thrown no light on the cause of the disease, or revealed any thing worthy of fixing the attention of physicians, for this *re-absorption* of the virus has been wholly unknown to them.

8. If a person be bitten by an animal believed to be rabid, the part under the tongue, where the tumours before spoken of occur, should be carefully examined once or even twice a day for six weeks, after which time the person may be considered as having escaped the disease if they do not show before themselves. Immediately on the appearance of these tumours they are to be *cauterized*, or what is perhaps still better, they are each to be freely opened with a lancet, which may be readily done if an assistant hold the tongue drawn up, and a little to one side by means of a napkin. There issues from these tumours a greenish sanious lymph, which the patient must spit out, and then thoroughly cleanse his mouth with a strong decoction of the *tops and flowers of the Genista luteo-tinctoria* (dyer's broom) prepared for the occasion. The patient is also to take daily during the last six weeks, a pound and a half of these *broom-tops* made into a strong decoction, or a drachm four times a day of the powder in substance, proportioning the quantities, however, in each case according to the age and constitution of the patient.

9. The operation of opening these tumours is really so simple and easy to perform, that it may be done by any person who has seen it performed once or twice, which renders this way of treatment equally beneficial in those parts of the country where surgical aid cannot be obtained. Should an occasion present, I offer myself to perform the operation in the presence of any one whom the government may be pleased to appoint.

In the year 1813, I resided in the Ukraine, in the capacity of physician to his Excellency the Count Morzezensky. The peasants of one of his villages (Kijawka) in returning home from their labours at evening in the autumn, were attacked by a large rabid dog from one of the neighbouring villages, and *fifteen* of them, of different ages and sexes, were bitten. As I lived five versts distance I received no intelligence of the fact until next morning, when I immediately proceeded to the place and provided for the unfortunate sufferers proper apartments, together with nurses and other attendants.

Whilst these arrangements were making, a deputation of old men waited upon me, and desired that I would permit the persons bitten to be treated by a neighbouring peasant who had been many years in the habit of treating such cases, and with invariable success. They assured me that they all could testify that this man had rescued many hundred persons in that country from the disease.

I had before heard of this man, and was desirous of satisfying myself on the efficacy of his method, which appeared to be of so great importance to humanity, whenever an opportunity should offer to witness the result. With the consent of the master of the village, I permitted the peasant to treat the cases, only reserving to myself one of them, in order to be convinced that the dog was really rabid, and on condition that all that he did, should be done in my presence. The case I reserved was a girl, aged six years, who was submitted to regular medical treatment; the others commenced taking the decoction of the *Genista luteo-tinctoria*, which the peasant made in my presence.

As I passed the most of my time near these unfortunate people, I was enabled in person to administer regularly, the remedies which I judged proper for the little girl, and I engaged a surgeon whom I had in attendance, to watch the peasant and not permit him to do any thing in my absence. He commenced by regularly inspecting under the tongue of

each patient, morning and night, and as the small tumours appeared, he showed them to me, and then cauterized them with a large needle, heated in the flame of a candle to a red heat. After this operation, the patient rinsed his mouth with some of the decoction which he was taking internally.

The little girl, whom I attended with the greatest assiduity, succoured with the best means the art afforded, became the victim of my experiments; for she was suddenly attacked with symptoms of hydrophobia on the morning of the seventh day after the accident, and died in my presence, eight hours after, in a most frightful paroxysm of the disease.

Of the remaining fourteen persons, twelve of them had tumours under the tongue, which were opened and successfully treated. The two others, who were last bitten, had no tumours. After using the decoction for six weeks, they were all dismissed in perfect health. I remained in that country for more than three years after this occurrence, during which time I saw all these individuals many times, and can attest that they remained perfectly free from the disease.

In the year 1818, I resided in Podolia, in a small village called Meskowka, district of Olgopol. In the month of February, *twenty-six* persons, Christians and Jews of different ages and sexes, were bitten by a rabid animal. The dog, after biting every one he met, suddenly disappeared, and was found shortly after in his usual retreat, lying dead on a wheat heap.

I was not enabled to discover the successive order in which these individuals were bitten. They were distributed into three divisions. The nine men I placed in one house, the eleven females in another, the six children in a third, with a Jewish surgeon to each establishment, to prepare and administer the decoction of the *genista luteo-tinctoria* and make a daily report to me of the state of their cases.

Result. In the first division, *five* had tumours under the tongue ; in the second, *all* had them ; and in the third only *three* of the children. In those that had been badly bitten, and in many places, the tumours appeared on the *third* day ; in the others on the *fifth, seventh* and *ninth* day. In one of the women who had been only slightly bitten on the right leg, the tumours appeared on the *twenty-first* day. The *seven* persons who had no tumours, continued the decoction during the six weeks, and were then dismissed with as many as had their wounds healed : the others remained until they were perfectly recovered. The *saeces* of the decoction of the *genista* I applied to the wounds in the form of a cataplasm, and found it an excellent detergent. In order to obviate constipation of the bowels, it is frequently necessary to give a gentle purgative once a week, or else a glyster of the decoction of *malva officinalis* with an addition of a small quantity of the *Astrakan-salt*.

It is important to know the premonitory symptoms that announce the appearance of the sublingual tumours of which we have spoken. The following are the only ones I have been enabled to discover ; dilatation and immobility of the pupils, dull and melancholy expression of the countenance, general *mal-aise*, and slight head-ache.

I have witnessed the complete success of this simple and easy method of treating persons threatened with hydrophobia, in near *forty* individuals. The fate of the little girl, who fell a sacrifice to my experiment, renders the character of the other cases more certain, and inspires greater confidence in the efficacy of means, which, I will venture to assert, will be attended with constant success. I have since, at different times and places, treated *six* other persons in the same manner, and with equal success ; one of them, a peasant who had been bitten by a wolfe which was pursued, discovered to be rabid, and killed. The tendons and ligaments of the ankle-joint were so lacerated, in this case,

that the man was confined to his bed for two months, but finally recovered with considerable deformity of the foot.

From the disclosures which I have made on the character of the hydrophobic virus, I think we may conclude ; that the disease is in the first instance *local* ; that its development may be prevented by a timely discharge of the morbid matter ; that after *re-absorption* has taken place, there is no means of obviating its terrible effects ; that the symptoms of the disease, such as constriction of the thorax, violent pain of the head, dilatation of the pupils, occasional paralysis of the tongue, convulsions, horror of liquids, and all the other nervous symptoms are the consequence of the re-absorption of the virus ; and that the connection of the nerves of the fifth pair, those which go to the tongue, the intercostal and cervical, with the nerves of the excretory ducts of the *sublingual* and *submaxillary* glands, the primitive seat of the deleterious action of the virus, furnishes an evident explanation of all the symptoms of the disease : let us hope that physiology will one day give fuller and more satisfactory details on the nervous connections and sympathies of these parts.

It only remains for me, gentlemen, to express my ardent desire that this memoir may be made public, in the hope that it may prove useful in the cause of humanity—the great object of my labours and wishes.—Accept respectable President and Members of the society, who have had the goodness to receive me as a colleague and collaborator, the observations, I offer you from motives of philanthropy—aid me with your practical experience and intelligence and let us hope at length, to stay the disastrous consequences of a bite of a rabid animal.

NOTE.—The Editors are indebted to the President of the State Medical Society, for this highly interesting document, who received it from his Excellency Mr. Middleton, the American minister, near the court of St. Petersburg.

Essay on a Humour of the Eye which is little known, and on the diseases produced by its deranged state. By L. L. JACOBSON, M. D. Professor, &c. Translated from the Latin of the Transactions of the Royal Medical Society of Copenhagen. By EDWARD W. WELLS.

IN the course of my anatomical examinations of the eye, I frequently observed a small quantity of serous fluid about the posterior part of the retina. This, I at first suspected to be collected there from transudation after death; but by a series of experiments made on eyes taken from the body as soon after dissolution as possible, I was furnished with good grounds to believe, that this humour was constantly present in a fluid state, and that it was necessary to the structure and functions of the visual organ.

The existence of such fluid has heretofore been decided upon by some anatomists. The first who noticed it from his own observation was Verle.* He remarks, than on puncturing the choroid coat near the optic nerve, he observed a watery fluid to be discharged, and ascertained that it flowed from the exterior and posterior part of that membrane; but whether this humour was a natural secretion, or only the result of putrefaction he does not take upon him to decide. Though his observation carries with it the appearance of truth, it is not considered satisfactory by Zinn,† who is of opinion that such humour is the result of disease, and contends that in a sound state, the

* As the work of this author is rarely to be met with, I may be allowed to give the observation in his own words. He says, "*Cum pungerem Choroideam prope nervum opticum, emanabat humor aqueus.* (p. 31.) *Ex hac tunica (choroidea) sumpius puncta, postquam eam excepi teneribus forcibus, semper ex parte exteriori et posteriori vidi promanare humorem aqueum, sive id sit ex natura, sive imputandum sit corruptioni, non est meum hic definire.*" *De anatomia artificiali oculi humani* (Amstelod 1650. p. 36.)

† *Descript anat. oculi hum.* p. 25.

choroid coat and the retina are in close apposition. This opinion was adopted by Haller,* and has been retained by all succeeding anatomists.

But mindful of the sound precept of Galen, that a knowledge of anatomy is not to be derived from books, but from actual dissection of the dead body, I take the liberty to dissent from the received opinion, and confiding in my own investigation, conclude that *a portion of watery fluid does exist between the choroid coat and the retina*. My business is therefore, to make a diligent inquiry into the situation of this humour, and to explain what may be conjectured concerning its nature.

In the healthy condition of the eye, this humour does not extend over the whole of the posterior surface of the retina but is only found on the side external to the optic nerve. It is contained between the choroides and the retina, and fills up the cavity of a plait formed by the latter membrane. The space that it occupies is surrounded by an arterial circle, which incloses the plait or fold, and its central foramen. A quantity of the like fluid, situated as above described, exists also between the retina and the vitreous humour. The inner surface of the fold, and of the retina within the before mentioned arterial circle, is likewise bedewed with the same.

This humour may be demonstrated as follows. The eye of a recent subject must first be carefully removed from the orbit; then, a cautious incision being made in the back part of the sclerotica, the choroid coat on the side external to the nerve is to be cautiously dissected off, and the posterior surface of the retina, with the cavity of the plait, will be exposed to view. All the region moistened by the fluid adheres less intimately than the other parts of the retina, to the choroid coat. By means of a slender pipe, the dissector must now blow into the cavity of the plait, so that the air

* Phys. T. V. p. 356.

which is expelled may strike its outer extremity which being dexterously performed, the air will easily find its way through the central foramen, pass between the vitreous humour and the retina, and collecting in the spot inclosed within the first circle of the central artery, will distend the membrane into a bubble. Now the retina outside of the above circle, adheres more strongly to the vitreous humour, and cannot easily be separated by forcing air through a puncture made in the coat itself.

Not to neglect any thing which might throw light on this difficult inquiry, I had recourse to congelation, as practised by Lyser and Petit. In repeated experiments, I observed that there had began to form between the choroid coat and the retina, a frozen lamina of a peculiar structure. This lamina, situated on the side of the eye external to the optic nerve, extended outwards and forwards, and was composed of lanciform crystals proceeding in the form of rays in the same direction, the optic nerve forming, as it were a focus. This structure differs very materially from the appearance which the vitreous humour has when frozen. Although I am aware that the above mode of investigation is not the most satisfactory; yet, the constant result of these experiments appears to favour my opinion concerning the existence of a fluid between the choroid coat and the retina.*

As the before mentioned plait of the retina, with its central foramen and yellow border, is peculiar to the human species, and is not found in the eyes of other animals, the ape and swine perhaps excepted, I have not a little regretted the want of some zootomical or comparative inquiries, which might confirm or invalidate my observations. Believing, therefore, that the pains which I myself have bestow-

* I made experiments of this kind at Leipsic, in the winter of 1813—14, when the coldness of the weather, and an abundance of subjects, afforded me a most ample opportunity for this and other anatomical investigations.

ed upon this subject, are worthy of some consideration, I shall briefly relate the discoveries I have made. In the eyes of most animals, is to be observed the pyramidal process described by Home, which penetrates into the body of the vitreous humour: within this process is contained a certain portion of watery humour, which appears to exist also on the posterior surface of the retina. But on account of the dissimilarity in the organization of man and brutes, I have hesitated to compare this fluid with that of the human eye, and, as the before mentioned structure of the retina is peculiar to the human species, I do not think it improper to conclude, that the humour moistening that part is also appropriated solely to man.

Such is the information I have derived from the healthy state of the eye. Although these facts have long been known to me, I have been backward in laying them before the public, from a conviction, that the fluid could only be demonstrated by those who gave as much attention to the subject as I had myself bestowed upon it.

At length however, I had occasion to examine the diseased state of this humour, and thus by the aid of morbid anatomy, every doubt of its true nature and use was removed from my mind. When this fluid becomes redundant, or altered from its natural condition with regard to quality it produces two changes in the eye of the greatest moment.

When the quantity is increased, it produces a disease, formerly scarcely known, but latterly noticed and described under the name of *Staphyloma posticum*.

This disease has been taken notice of by no one, except Scarpa, a man endowed with uncommon talent and acumen for anatomical investigations; and it is by him described in his excellent work on the diseases of the visual organ.*

* Practical observations on the principal diseases of the eyes, translated by James Briggs, Lond. 1806. 8vo. p. 433.

In the eyes of two dead subjects, this author observed a protuberance on the posterior part of the sclerotica, externally to the optic nerve, whence his denomination of the disease. But it is to be regretted, that the examination of the internal parts is so briefly described, that nothing can be collected from it, relative to the nature and condition of the disorder.

While residing in Paris, for the purpose of prosecuting my studies, I also had an opportunity of observing two cases of this affection, and I then presented my observations on the subject to the society of the Medical Faculty, as will appear from their minutes.

The first case I saw was in one of the eyes of an old female subject, the globe of which was so altered in shape as to be perfectly pyriform. The sclerotica, on the side external to the optic nerve, where the ciliary vessels enter, formed a protuberance having a pretty broad base and projecting about two lines from the general surface of the eye. On dissection, I found an aqueous humour between the choroid coat and the retina, which, with the annexed vitreous humour, was pushed considerably forwards. Hence, the retina was of a funnel shape, and the vitreous humour appeared to be diminished in bulk.

In the second case, I found the eyes of an elderly man affected in the same way. In this instance the balls were more pyriform, and the protuberance was larger and had a broader basis. In one eye, the quantity of fluid collected between the choroides and the retina was much larger than in the other, and consequently the space between the coats was greater. The vitreous humour appeared more compressed, and much diminished in volume. The lens of this eye was opaque, and had degenerated into a cataract.

From the foregoing facts we may infer, that the disease, denominated by Scarpa, *Staphyloma posticum* is the result of a

morbid accumulation of that humour, which naturally exists about the posterior part of the retina, and that it constitutes a peculiar species of hydrophthalmia.

The cause of this, as of other hydropic affections, must be referred, either to increased secretion, or to diminished absorption, which again depend on an affection of the organs governing those functions.

It is not easy to decide whether this humour be secreted by the retina, or by the choroid coat; for they appear equally adapted for the secretion, and equally affected by the disease. However, as in a large accumulation of the fluid, the retina is found to be in a somewhat degenerated state, I think it most probable that it is principally concerned in the secretive process.

This humour, then, being secreted in a greater quantity than natural, is accumulated between the choroides and the retina, and presses forward the latter coat along with the vitreous humour; but while the secretion is increased here, it is proportionally diminished in the other part of the eye, whence the cause and origin of the diminution of the vitreous humour in bulk, and of the opacity of the crystalline lens.

At length, the secretion still going on, the collected humour acts by pressure upon the choroid coat and the retina, which being unable to resist, are expanded and form the protuberance on the side external to the optic nerve, whence the name given to the disease by the illustrious Scarpa. It is probable that the form and size of this protuberance answer to the condition and violence of the disease.

Hence, we are at liberty to suppose that this affection may exist without producing any such tumour. which opinion is supported by facts, as well as by the testimony of medical writers. For, though no one except Scarpa has spoken of such a disease, yet we find in authors who have treated of the *synchisis* or effusion of the vitreous humour, remarks

which seem to appertain to this subject. From among others we shall select those of Morgagni.* He says ; “ Immediately on making an incision in the back part of the sclerotica, a limpid water flowed out, in which a great part of the vitreous humour appeared to have been dissolved ; while the *remaining part*, retaining somewhat the natural appearance, remained connected in the usual manner with the crystalline lens.” Is it not evident enough, that in this case the fluid behind the vitreous humour had accumulated, compressing the latter, which was thereby diminished in bulk and at the same time pushed forwards ?

Hence Morgagni† and Zinn‡ justly conjectured, that the retina must necessarily be pressed forwards by the humour thus morbidly collected in the above mentioned situation.

Whether the function of vision is disturbed by this disease, and if so, in what manner ; whether we can ascertain the existence of the disorder in a person labouring under it ; whether the name given to it by Scarpa is an apt one ; are questions which I must defer to another opportunity ; and not to break at present the thread of my discourse, I proceed to consider some other diseases belonging to the same class. These are, ossifications, which are sometimes observed in the eyes, and petrifications, as they are called, of the humours.

Concerning these affections we meet with many observations in medical authors.|| Some believed, that in these cases, it was the retina, others, the choroides that was affected ; and where a large calcareous mass was found in the cavity of the eye, they concluded that it was an ossification or petrification of the vitreous humour. Scarpa also has some

* De sed. et caus. morb. Epist. xiii. p. 6.

† Ep. anat. xviii. p. 38.

‡ L. c. p. 25.

|| Vide Hildanus, Blassius, Lancisi, Heister, Morgagni, Morand, Henkel, Malacarn, Haller, Pellier.

accurate remarks on this disease, which I shall here take the liberty of quoting entire.

“ Among the very considerable number of diseased eyes, which the friendly condescension of Dr. Monteggia, a celebrated physician and surgeon of Milan, has afforded me an opportunity of examining, I have found one almost entirely transformed into a stony substance.

“ This eye, taken from the body of an elderly woman, was about one half the size of the sound one. The cornea was dusky, behind which the iris appeared of a singular figure, being concave, and without foramen or pupil in the middle. The rest of the eye-ball, from the termination of the cornea backwards, felt unusually hard to the touch.

“ By making an incision I found the sclerotica and the choroides nearly in a natural state, and a small quantity of limpid fluid issued from the anterior chamber of the aqueous humour. Beneath the choroid coat there appeared two hard calculous *scutellæ*, united together by means of a compact membranous substance, one of which was situated posteriorly, the other anteriorly. The former occupied the bottom of the eye; the latter the situation of the corpus ciliare and the crystalline lens.

“ Having made an incision through the membrane, which united the margins of the two calculous *scutellæ*, I found within the cavity, instead of the vitreous humour, some drops of a glutinous bloody fluid, and along the axis of it, a small soft cylinder, which running anteriorly from the bottom of the eye along the greater axis of the ball, went to be implanted in an elastic cartilaginous substance, situated in the centre of the anterior calculous *scutella*, precisely at that part, which in a natural state is occupied by the crystalline lens and its capsule, both of which parts were entirely wanting.”*

* L. c. Chap. xi. pp. 532, 533.

According to the investigations which I have been able to make, these two diseases belong to one class, and differ only with respect to the matter secreted. For the cause of both is a degeneration of the before described humour, and a deposition in its place, of a calcareous substance. When this is formed in lamellæ which adhere to the choroides, whence probably it derives its origin, it may easily be mistaken for an ossification of that tunic; but a more minute examination will show that this calcareous or osseous crust or lamina is only adherent to the internal surface of the choroid coat, and may be separated from that membrane. I have already had frequent opportunities of satisfying myself with regard to the formation and seat of such concretions. I have always found them on the posterior and exterior part of the eyeball, between the choroides and the retina, adhering principally to the latter.

The greater number of eyes thus diseased, which I have dissected, were in an atrophic condition; though I have found the same kind of concretion also, in a cancerous state of the organ. A specimen of this sort of calculus I owe to the kindness of the eminent and lamented Le Gallois, who on the day when I presented to the Society of the Medical Faculty of Paris, my observations on the *Staphyloma posticum* so called, showed me the eye from which I had taken that specimen. I found this concretion of much service to me in my subsequent investigation of the subject.

What has been called a petrification of the vitreous humour, is nothing more than the formation, or generation of a calcareous mass, collected or deposited in large quantities in that humour.

This opinion is supported by the situation of this concretion between the choroid coat and the retina, by the scutelliform figure it assumes, and by the aperture with which its middle lamina is perforated.

Seventeen years ago, I met with a calculus of this kind in

one of the eyes of a man who died of old age. It is perhaps one of the largest size.* I now regret I did not at that time examine with sufficient care, all the phenomena of the affected organ, but too hastily extracted the concretion, which I then considered an ossification of the vitreous humour. This calculus is of a compact texture, and probably consists of phosphate of lime. Its figure is that of a segment of the eye. Its posterior surface is convex; on its anterior surface is to be observed a funnel shaped cavity which opens into a canal on the posterior part.

I formerly supposed that this cavity was owing to the lens, and this canal to the central artery; but it is more likely that the retina, extended and attenuated, had passed out thro' the canal, and that the funnel-shaped cavity had served for the reception of the degenerated vitreous humour. What renders this probable is, that the form of the concretion agrees with that of the space between the choroid coat and the retina, which, in the disease before described, contains the accumulated watery fluid.

But, by the observations of the illustrious Haller and Scarpa, my conjecture appears confirmed beyond a doubt.

The former writer, describing a cup-like calculus in the eye, says, that the nerve passed through a foramen in this stony formation, though he does not positively decide whether it was the retina, or some other part, which was converted into osseous matter. He appears, however, to admit that it was the former.†

* This is still preserved in the anatomical collection of the Royal Academy of Surgeons.

† It (the cup-like calculus) was perforated with a round hole, through which the optic nerve passed, which gave it still more the appearance of the retina indurated. Within this bony cavity I found no genuine vitreous humour, but a kind of nerve as it were, viz. a white cylinder, which passing by the above foramen through the whole diameter of the bony hemisphere, was attached to a confused bony substance, which might be considered as the degenerated crystalline lens. *Pathological Observations. By Dr. Albert Haller, Lond. 1756 Obs. LIII. p. 164.*

But a stronger argument in support of my opinion is the observation of Scarpa before quoted, from which it appears, that this accurate observer found, between the choroides and the retina, a concretion similar to the one which I have described. In like manner it was excavated on its anterior surface, and perforated by a canal in the bottom. Through this canal the degenerated retina extended to an elastic mass in the cavity of the calculus. The author thus continues :

“The optic nerve degenerated into a thread, passed through the sclerotic and choroid coats, advanced through the centre or bottom of the posterior calculous *scutella*, and was lost in the small soft cylinder, which, as I have stated, went to be inserted in the cartilaginous substance, situated in the centre of the anterior calculous *scutella*, or the part which is naturally occupied by the crystalline lens and its capsule.”†

I trust that I have now plainly demonstrated that the concretions found in the eye are seated between the choroid coat and the retina, and whether formed in the shape of calcareous lamellæ or harder calculi, that they are of the same nature, and are not to be considered as an ossification or petrification of the vitreous humour.

These diseases are derived from the same source as the before described watery accumulation between the coats of the eye, and differ only with regard to quality. The humour secreted, is either so loaded with earthly particles, that it quickly covers the choroides with a crust, and thus forms an osseous lamina, or else, saturated with these earthy particles, it is collected in so large a quantity, as by its pressure to injure the texture of the eye, whence the atrophic state of the organ. Then, the fluid particles being absorbed, the earthly part remains, and puts on the form which we have described.

† l. c. p. 534.

From the foregoing investigations the following inferences may be drawn :

1. That an aqueous humour exists between the choroides and the retina, inclosed in the cavity of a plait of the latter membrane.
 2. That a portion of this humour is found also on the anterior surface of the same part of the retina.
 3. That this humour is sometimes increased in quantity ; that this accumulation in the first place pushes forwards the retina and the vitreous humour, and afterwards acts on the choroides and sclerotica, which being expanded, the disease is produced, denominated by Scarpa staphyloma posticum.
 4. That this fluid may also be altered in quality ; and that it then gives origin to calculous concretions of a different nature, which hitherto have been erroneously considered ossifications of the coats of the eye, or of the vitreous humour.
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Observations on the use and abuse of Mercurial Medicines in various diseases. By JAMES HAMILTON, JUN. M. D. fellow of the Royal College of Physicians, and Professor of Midwifery in the University of Edinburgh. With Notes and an Appendix, by ANSEL W. LIVES, M. D.—8vo. pp. 216—Bliss & White.

The title of this book, independent of the established reputation of its author, or the intrinsic value of the performance, is amply sufficient to recommend it to the attention of every reading physician. There is scarcely a medical man whose education bears date within the last forty years, who will not find himself immediately interested in the subject of which it treats, or who will not feel himself rebuked by the doctrine which it teaches ; for if there is any one article of the *Materia Medica* which can claim precedence of the rest in the universality of its application as a remedy, assuredly

It is mercury in some of its varied forms, and there are few indeed of those who have been accustomed to prescribe it, who would not feel themselves offended by any intimation that they were either ignorant of its operation, or heedless of its ultimate consequences; and yet one or other of these imputations is justly chargeable upon the largest half of the profession, if the facts and reasonings of Dr. Hamilton are admitted to be conclusive. We have no disposition to forestal the opinions of our readers by any of our own remarks, but would much rather that each should judge for himself after an attentive perusal of the book, which possesses merit of no common order; a merit the more extraordinary, since its very faults can in nowise detract from its extensive usefulness. But as it is hardly to be expected that it should be universally read, and as we are well aware that many of our brethren would prefer that others should read for them, we purpose to give an analysis of its contents. In the outset we will observe, that we do not recollect any medical work which comprises, in the same number of pages, more materials for rigid practical scrutiny, and more points for serious reflection, than the one now before us, and though it is not without faults, the very fact, that the opinions maintained by the writer are urged with a confidence which leaves no doubt of his sincerity; is alone calculated to arrest attention and to challenge examination—that the book will circulate extensively, we have little doubt, and certainly none, that its tendency in the main will be highly useful.

If the introduction to any work, whenever it is appropriate, must be presumed to contain not only the intentions of the writer, but a fair sample of the manner in which such intentions may be expected to be executed; Dr. Hamilton's preface will appear to be the most exceptionable part of his book, since he avows his object to be to prove *that infinite injury is caused by the exhibition of mercury, and that safe substitutes ought to be found in all diseases where violent remedies*

are not absolutely required ; thereby intimating in language too plain to be misunderstood, that its use should be proscribed in all those complaints which are manageable by any other means, or in other words, that it should be considered as the *resort dernier* : a declaration more calculated to startle the timid practitioner into a wary examination of his opinions, than to prejudice the reader in their favour. That he reprobates the practice of the physicians of Great Britain in respect of this remedy, is sufficiently apparent from the following paragraphs.

“ There can be no doubt, that many practitioners of the first respectability prescribe on every trifling occasion calomel, or the blue pill. Thus, calomel is now in Great Britain almost the universal opening medicine recommended for infants and children, and a course of the blue pill (which is one of the mildest preparations of mercury) is advised, without any discrimination, for the cure of trifling irregularities of digestion in grown persons.”

“ Dr. Falconer of Bath (in a paper inserted in the First Volume of the Transactions of the Medical Society of London, dated May 1809) has in strong language reprobated this practice, and has pointed out many of the dangerous effects of the indiscriminate use of mercury. His warning voice, however, has not been listened to ; for the employment of mercurial medicines has, for several years, become more and more extensive.”

“ But when the effects of mercury upon the human body are accurately investigated and duly considered, it cannot fail to appear, that infinite injury must accrue from its use in many cases, and that whenever, from the nature of the indisposition, violent remedies are not absolutely required, a safe substitute for so hazardous a medicine ought to be found. It is the object of the author, in the following pages, to illustrate those propositions ; and in doing so, he readily avails himself of the recorded facts and observations of those respectable members of the profession, to whom proper deference is due.”

The author divides his book into eleven sections ; the first of which he devotes to the consideration of the effects of mercury.

2d. Its powers in subduing the syphilitic virus.

3d. The cautions necessary during its use.

- 4th. The means of relieving its morbid effects.
- 5th. Its use in diseases of the liver.
- 6th. Its use in affections of the stomach.
- 7th To the consideration of an affection of the duodenum, resembling chronic hepatitis.
- 8th. Treats of affections of the spleen and pancreas.
- 9th. Of the use of mercury in affections of the intestinal canal.
- 10th. Of its use in dropsical affections.
- 11th. Of its use in croup and in inflammation of the Iris.

1st. *The effects of mercury.*—After premising a few remarks on the varied susceptibility of different constitutions to the effects of mercury, he concisely enumerates the most obvious results of its duration, viz : increased action of the heart and arteries ; the increased determination to the sub-maxillary glands ; emaciation ; debility, and excessive irritability of the whole system : and draws a parallel between inflammatory, and what has been called mercurial action, which agree in some, while they differ in other, very material points. The two most remarkable, according to Dr. Hamilton, are, that the one more immediately tends to produce congestion, while the other produces the very opposite effect of increased secretion ; and the other is, that the deficiency of muscular strength, a necessary attendant on both conditions of the system, is by no means proportional ; the mercurial fever being most distinguished by consequent debility : and besides, mercurial fever is attended by injurious effects not referrible to increased action merely, but which are considered specific or sui generis. Reasoning upon this subject, he says it might be concluded,

“ That if there be an inordinate action of the heart and arteries, attended with an altered state of the blood and with debility, while the increased secretions accompanying this inordinate action have no tendency to allay it, the health must be rapidly undermined ; and if there be ulcerations in any part of the body, they must

as certainly degenerate into malignant sores, as blistered surfaces or scarifications mortify in cases where the living powers are much exhausted."

"Experience has proved the reality of such conclusions, but prejudice and inaccurate observation led many practitioners of deserved reputation to attribute those effects of mercury to other causes, till Mr. Mathias published his valuable remarks on what he terms the mercurial disease. Thus, before Mathias's publication, the injurious effects of mercury in some syphilitic cases were attributed to the original virus operating on scrofulous, or cancerous, or scorbutic constitutions, or to some complication or anomaly which was inexplicable."

"Mr. Mathias has unequivocally shown, that certain dangerous changes upon ulcerations originally syphilitic, and certain derangements of health, occur wherever mercury has been administered in too acrid a form, or in too large a quantity; and his remarks are confirmed by the experience of every practitioner who has, with extensive opportunities of observation, been attentive to the phenomena. He imagines that the action of the mercury in such cases is of a specific or peculiar nature; it more probably, however, is merely in an inordinate or excessive degree, and in no other respect different from what it is in every case.* It is a well known fact, that exposure to cold, bodily fatigue, and irregularities of diet, particularly indulgence in intoxicating liquors, have aggravated the severity and malignancy of syphilitic ulcerations, whether primary or secondary. But as all those different causes concur only in one respect, viz. in exciting inflammation, it is evident that mercury, when it affects the system, must be productive of equally injurious changes upon the ulcerations in question and upon the general health, because it probably induces a more violent degree of inflammation, than exposure to cold or irregularities of diet."

"Upon the same principle may be explained the fact noticed by all practical writers, that scrofulous sores, and scirrhus tumours, and cancerous affections in certain stages of their progress, are much aggravated by preparations of mercury."

After noticing the effects of salivation and emaciation, the author thus sums up the consequences of mercurial debility—

"In some, temporary delirium takes place, in others, palsy or epilepsy supervene, and in many the memory and judgment are—

* Vide Mathias on the Mercurial Disease, 8vo. Lond. 1810, *passim*.

more or less permanently impaired. Instances, too, have occurred, where sudden death has supervened, apparently in consequence of a very trifling exertion or agitation. Mr. Pearson has well described such cases under the title of *Erethismus*. He says this state "is characterised by great depression of strength, a sense of anxiety about the precordia, irregular action of the heart, frequent sighing, trembling, partial or universal, a small, quick, and sometimes intermitting pulse, a pale contracted countenance, a sense of coldness; but the tongue is seldom furred, nor are the vital or natural functions much disordered."

"It may be alleged, that these are extreme cases, and it must be admitted, that in many instances, those very violent effects do not follow. Delicate individuals, however, particularly those who have been accustomed to a sedentary life, and, therefore, in an especial degree, females, generally experience after a course of mercury, various modifications of disordered feelings, communicating the idea of imaginary diseases, which unfit them for the duties of life, and render existence a burden."

"Among the anomalous complaints arising from this cause, may be enumerated, impaired or capricious appetite for food, with all the ordinary symptoms of indigestion, particularly retchings in the morning, and flatulency—disturbed sleep, with frightful dreams—impaired or depraved vision—frequent pains and aches in different parts of the body—occasionally such sudden failure of strength, as if just dying, and at other times violent palpitations at the heart, accompanied with difficulty of breathing. Along with all these complaints, there is such a wretchedness of look, with such a propensity to brood over their miserable feelings, that it is extremely difficult to persuade the relations or attendants of the patient that there is no serious indisposition. Indeed medical practitioners who are not accustomed to weigh with mature deliberation all the complications of symptoms, are generally deceived in such cases, and involuntarily add to the alarm both of the unhappy sufferer and of the attendants."

"I might cite all writers on the *Materia Medica*," says Dr. Falconar, in the paper alluded to, (page 110) "for authorities that the long continued and frequent use of mercury is not free from danger; that among other ill effects, it tends to produce tremors and paralysis, and not unfrequently incurable mania. I have myself seen repeatedly, from this cause, a kind of approximation to these maladies, that imbibited life to such a degree, with a shocking depression of spirits, and other nervous agitations with which it was accompanied, as to make it more than commonly probable, that many of the suicides which disgrace our country, were occasioned by the intolerable feelings that result from such a state of the nervous system."—To the truth of these remarks every un-

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prejudiced physician who has been in extensive practice must bear testimony."

"Such are the ordinary and well known effects of mercury when given in sufficient quantity to act upon the human body—but in many cases other deviations from health ensue."

"Of these, the most common is excessive diarrhœa, accompanied often with discharges of blood from the bowels. This is so apt to occur in some individuals, even though the mercury be administered by being rubbed upon the surface, that every writer upon lues venerea has mentioned this effect as one of the great obstacles to the cure of the disease."

"The random experiments of speculative physicians upon patients labouring under scrofulous affections have proved, that in some cases ulcerations of the soft parts, and caries of the bones, originally arising from ill-conditioned states of the system, are much accelerated in their progress by mercurial medicines. Of this many melancholy examples might be cited. A boy about eleven years old had a sore on one cheek, with an affection of the jaw, which were attributed to the mismanagement of a dentist in extracting a carious tooth. A physician was consulted, (after the patient had suffered for some months,) who immediately prescribed a regular and full course of mercury. In a short time ulcerations in the throat appeared, the nose sunk, and one of the eyes was nearly destroyed, while the general health became so seriously injured, that death followed in a few months. Can it be for a moment doubted, that all those morbid changes proceed from the inflammatory action of the mercury?"

2d. *The power of mercury in subduing the syphilitic virus.*—After what he has just seen, the reader will be inclined to believe that the author purposes to show his unqualified disapprobation of the remedy in question under all circumstances; but this is far from the fact, for in this section, he appears in the character of an avowed advocate for its exhibition for the cure of a disease, in the treatment of which, physicians have not always been agreed. The protean forms which syphilis assumes, have given origin to a great variety of opinions respecting the most successful methods of combatting or controlling its malignant consequences, and it might be reasonable to expect, that a writer who certainly has no prejudices in favour of the medicine, and who is dis-

posed to consider its most salutary effects as countervailed by no ordinary degree of hazard, should range himself on the side of those who view it as a doubtful efficacy ; but Dr. Hamilton on this question has no doubts to be dispelled, nor compromise to offer ; and with a full knowledge of all the facts adduced in favour of treating this disease, by means other than mercurial, he enters his solemn protest against any attempts to cure it without its assistance. And it must be confessed, that in his strictures on Mr. John Hunter, who denied that children could be born infected, unless by contact with the diseased parts of the mother, he has brought together such a body of evidence as must convince the most sceptical, that confidence in a radical cure, cannot be expected through the agency of any other known means. Our limits not permitting us to extract this part of his book without mutilating it, we can only recommend it to the attentive perusal of every practitioner who still has doubts on this subject.

3d. *Cautions necessary during the use of mercury.*—The remarks under this head, though not new, are yet so interesting, that we shall transcribe them entire.

“ The first precaution to be adopted in this climate during a course of mercury, is confinement within doors, with a regulated temperature of the apartments. Both the utility and the necessity of this precaution must be obvious, from the remarks already made, that it is unnecessary to expatiate on the subject : not that it is meant that the patient should be confined to an ill-ventilated room ; for, on the contrary, a plentiful supply of fresh air is of essential utility. While the boldness with which Mr. Pearson exposes such patients to cool, dry, open air, may be well suited to persons who have been immured in a crowded hospital, with a mercurial atmosphere, it certainly would be most prejudicial to the better ranks of society in private practice.”

“ *Secondly*, The diet ought to consist of the mildest possible food, such as preparations of milk and farinaceous matters, with weak animal mucilages. In short, all stimulant food, or drink of every description, ought to be most scrupulously refrained from.”

“ *Thirdly*, If the individual be robust, sixteen or twenty ounces

of blood should be drawn from the arm before any preparations of mercury be exhibited. Where, from the delicacy of the patient, blood-letting cannot be advised, confinement within doors, and low diet, should be persevered in for at least a week previous to beginning the mercury, and during that time one or more doses of cooling physic ought to be taken."

"*Fourthly*, The mercury must not be given in such quantity, or with such activity as to produce a sudden effect upon the system. This is certainly one of the most practical improvements, suggested by Mr. Abernethy and others, and confirmed by the late experiments; for irreparable mischief was often committed by the hurry with which the system was loaded with mercury. If the other precautions be implicitly adopted, the more slowly the mercury is administered, the more certainly, and perhaps speedily, will the primary sores heal."

"*Fifthly*, Although in particular cases some of the more active mercurial oxydes may be useful, the blue pill or the blue ointment furnish in general the safest and mildest preparations of mercury."

"*Sixthly*, Salivation is to be guarded against, by lessening the dose, or suspending the medicine, whenever the brassy taste in the mouth is perceived. The same measures are to be pursued if any irritation of the bowels threaten."

"*Seventhly*, Some vegetable diluent ought to be drank in large quantities, for the purpose of preventing the peculiar state of the blood which mercury is so apt to produce. The decoctions of sarsaparilla, guaiac, sassafras, &c. answer this purpose, and perhaps they are all equally efficacious, if drank tepid, and in sufficient quantity."

"*Eighthly*, It is extremely difficult to establish any general rule for the duration of a mercurial course, as that must be regulated very much by the circumstances of each particular case. From two to three months may perhaps be sufficient in the majority of cases."

"*Ninthly*, The daily use of the warm bath, where that can be conveniently commanded, is found particularly beneficial."

"*Tenthly*, If any irritable feelings occur while under the use of mercury, the use of the medicine should be instantly suspended, and the most active measures for checking the progress of such complaints ought to be carefully adopted. Preparations of camphor, of the spiritus ammoniæ aromaticus, of opium, of cicuta, &c. are severally useful according to the circumstances of such cases."

"*Eleventhly*, After the mercurial course is finished, the patient ought to remain within doors at least a fortnight, improving the diet, (though still abstaining from wine and stimulating liquors,) and taking gentle exercise, progressively increasing it according to the return of strength."

“*Lastly*, The flannel and woollen dress, in which those under a course of mercury should be (literally speaking) encased, is to be changed daily, and besides the ordinary precaution of having those articles of dress well washed, it is necessary that they be exposed for at least twenty-four hours in the open air, and afterwards to the influence of a large fire, before being again used.”

4th. *The means of removing the morbid effects of mercury.*—On this subject our author adds but little to our stock of information, but bears testimony to the correctness of the views of Hunter, Abernethy, Pierson, and others, who have given ample directions to fulfil this indication.

5th. *The use of mercury in diseases of the liver.*—This article in any hands presents a wide field for observation, and perhaps admits of a greater latitude of discussion than any other which the Dr. has thought proper to present to his reader, we therefore feel the greater regret that it has not received a larger share of attention. Complaints of the Liver are among the most frequent as well as the most dangerous maladies to which man in civilized life is subjected. In addition to its idiopathic affections, which constitute a very large proportion of the diseases of warm and temperate climates, it is liable from its very size, structure and functions, to be involved in the derangements of other organs, from causes whose operations are intermediate: its functions are perhaps more frequently impaired than those of any other part of the human system; and all its morbid conditions are pregnant with a greater variety of serious consequences. For these reasons, independent of any which might be drawn from the various modes of treatment which have, at various times, been recommended for hepatic affections, we regret what we cannot but view as a great deficiency. The substance of the writer's remarks may be condensed in very few words; he acknowledges the efficacy of mercurial medicines in these affections as they occur in warm climates, and admits the necessity which calls for their exhibition in large quantities to arrest complaints which, from their vio-

lence of symptoms, and rapidity of succession, tend speedily to a fatal termination. But believing that in the climate of England "the change of structure of the liver, which eventually ends in suppuration or scirrhusity, is found to arise only from mismanaged intermittents, or from scrofula, or from the abuse of intoxicating liquors," he inclines very much to restrict its use, for he says—

"The ordinary mode of exhibiting mercury for the cure of chronic hepatitis in this country, not unfrequently hurries on the disease, or, by impairing the constitution, lays the foundation for paralytic affections, and it may be truly affirmed, that it thus often shortens life. As there are several cases on public record which justify this remark, the author need scarcely offer any illustration."

"It must therefore be evident, that in certain stages of congestion of the liver, as well as of other viscera, the inflammatory diathesis excited by the use of mercury, instead of removing the congestion, accelerates the diseased changes which had begun."

"Every conscientious practitioner will allow, that in many cases the most minute and deliberate investigation is insufficient to decide on the true nature of the case, as is more fully explained in the following sections—and hence it is of the utmost importance to proceed cautiously in the exhibition of so powerful a medicine as mercury."

"Instead, therefore, of directing mercurial ointment to be rubbed upon the side, and at the same time the blue pill or calomel to be taken internally, so as to affect the mouth as soon as possible, and keeping up the soreness of the mouth for many weeks, the prudent practice is to combine the mercurial with antimonial preparations, and with occasional doses of neutral salts in a very dilute form, so as to increase the secretion from the skin and from the kidneys, without at all affecting the mouth or irritating the bowels. Along with these medicines, a diet consisting of weak animal mucilages, and of the lightest farinaceous matter, ought to be advised, and warm clothing, and strict confinement within doors, should be enjoined. The daily use of the warm bath is also to be recommended."

"In those violent degrees of chronic hepatitis, where bleedings at the nose or other hæmorrhages attend, or where dropsical symptoms, with foul blotches of the skin, have supervened, there can be little other resource than palliative means, adapted to the sufferings of the individual."

Use of Mercury in affections of the stomach.—In these dis-

eases, Dr. H. unequivocally condemns the exhibition of mercury in any form, contending, that when they merely consist in derangement of function its use is injurious, and calculated to aggravate the complaint; and that when they are attended with alteration of structure the cases are hopeless, and, therefore, with much more propriety treated by palliatives.

Of an affection of the duodenum, which imitates chronic Hepatitis.—This complaint, it must be confessed, is not marked by any infallible diagnostics, at least if there are any, they are not very obvious; and hence it may readily be confounded with other affections of the chylopoietic viscera, with which it has an intimate relation. It may even be doubted whether it ever can exist without being accompanied by derangements of the digestive functions which are not referable to it as a cause; and never having seen the disease, or read any description of it in which it appeared to be idiopathic, we incline to consider it as a sequence of diseased liver, or chronic inflammation of the stomach. Dr. H. allows it to be difficult of detection, and in forming his opinion of its existence places more dependance upon the previous history of the patient's indisposition, than upon the presence or absence of any set or symptoms which may be supposed to characterize it.

“In doubtful cases, where, along with lassitude, sallowness of the complexion, occasional sickness, total want of appetite, thirst, irregular state of the bowels, occasional palpitation of the heart, flatulency, frequency or irregularity of pulse, dull pain in the right side, extending to the top of the shoulder, aggravated by pressure, by exercise, or by lying on the left side, and becoming occasionally acute after taking food, with loss of flesh, there be a daily evacuation from the bowels exceeding considerably in the quantity of real *stercus* the food actually swallowed, the author considers the disease to depend upon the state of the duodenum, if there be no tangible hardness or enlargement of the liver. When along with the same symptoms, there is a milky white appearance of the urine, as if it were mixed with chalk, he never has any doubt on the subject, because he has invariably found the duodenum affected under

such circumstances, and he never observed the same appearance of the urine in diseased liver. He owns that in several cases where the duodenum was certainly affected, that peculiar state of the urine did not attend."

"In some of the cases to which he has been called, the practitioner in previous attendance had supposed the *fæces* to be of a chalky colour, in consequence of having been covered with the urine, and it was necessary to separate the two excretions before the error was acknowledged. The expression of the *fæces* being covered by the urine, is intended to mark the fact, that the *fæces* in those cases form an adhesive mass on being passed, and therefore are literally covered by the urine discharged at the same time and not at all mixed with it. In every case hitherto under the author's charge, this relative state of the two evacuations has not been altered by the operation of any of the purgatives."

"The bright green colour of the urine mentioned by Dr. Ferriar, as preceding the exacerbation of pain in affections of the duodenum, has not hitherto fallen under the notice of the author of these pages. In some cases, the pain in the duodenum is characterized by its occurring commonly about an hour after eating, by its being different from that arising from distention of the stomach from flatulence, and by its not being relieved by the expulsion of wind by eructations."

Mercury, according to our author, aggravates this complaint, and therefore makes up no part of his treatment, which consist in slight cases of repeated doses of rhubarb, &c.

"Repeated doses of rhubarb, combined with magnesia or other laxatives, and a course of the white oxyde of bismuth, with mild food, are generally sufficient. But in the more violent degrees, it is necessary to begin with the application of leeches to the epigastric region, after which one or more antimonial emetics must be directed, and nauseating doses of the same, with occasional laxatives ought to be continued for some time, the patient being kept all the while upon the lowest possible diet, with respect both to the quality and the quantity of the food. After the urgent symptoms have subsided, the same mode of diet as that recommended for the cure of dyspeptic complaints ought to be adopted, and the white oxyde of bismuth combined with some aromatic, is to be advised."

The eighth section on affections of the pancreas and spleen seems to have been introduced merely with the view of giving the author an opportunity of expressing his conviction

that mercurial medicines if not *inadmissible* must yield in efficacy to safer means, such as Cheltenham water, cream of tartar, muriate of lime, and antimonials.

For the use of Mercury in affections of the intestinal canal Dr. Hamilton is by no means an advocate, and from the tenor of the preceding remarks, the reader has no reason to expect that he should be ; as a cathartic or as entering into the composition of purgative medicine, he unequivocally reprobates it, for he lays it down as a principle “ that no preparation of mercury can be administered without the risk of some consequences ensuing, which could neither be intended or expected,” and although he would not be understood to alledge that there are no cases in which calomel or the blue pill ought to be employed, he thinks that ordinary diseases ought not to be treated by such powerful and *dangerous* means ; and if he be met by the remark, that experience does not justify his opinions, seeing that calomel is in very general use for diseases of infants, not only by practitioners, but nurses, and none of his apprehended ill effects ensue ; he answers that the ultimate effects of many medicines, and particularly the metallic oxydes cannot easily be traced by physicians, much less by ordinary observers, and that the influence of calomel upon glandular disease whether of the mesentery or lungs may be such as in its remote consequences may prove seriously alarming and difficult of controul. So far indeed, does Dr. H. carry his aversion to calomel as a cathartic that he says “ it may with perfect confidence be asserted that a safe substitute may be selected in nine cases out of ten in which that mercurial oxyde is now prescribed.” As an anthelmintic he observes, its use cannot be sanctioned except on the plea of necessity, and that no such necessity exists is obvious from the great variety of vermifuge medicines known to exist, which are perfectly safe in their operation.

The use of mercury in dropsical affections.—Under this head our author has drawn up a general, though very interesting,

history of dropsy, in which he adopts the distinctions heretofore made by Drs. Wells and Blackall, which, if closely examined without reference to the urinary test, will be found to be substantially the old distinction as arising from direct and indirect debility. In the latter variety, or that species of dropsy preceded by inflammatory action, a phrase by the way which is only to be understood relatively, he altogether condemns the exhibition of mercury; though he admits with much allowance, that in the former species, small doses of mercurial medicine, combined with a judicious selection of tonics, has been found productive of benefit. In that species of dropsy known by the name of hydrocephalus, as well as in that which involves disease of the ovaria, he believes the exhibition of mercury to be productive of much injury, and with respect to the first, that its use is at variance with etiology of the complaint, as laid down by the very persons who prescribe the remedy.

“But the modification of dropsy which seems at present to be most commonly managed injudiciously, is the HYDROCEPHALUS. Practitioners who differ totally in their explanation of the nature of the alarming disease, cordially agree in recommending mercury for its cure. Indeed, it would appear incredible, were it not that the fact could be proved by the clearest evidence, that practitioners who declare hydrocephalus to be incurable, invariably prescribe the most active exhibition of mercurial medicines.”

“In estimating the effects of remedies, scientific practitioners and ignorant persons often draw very opposite conclusions. The former make every allowance, both for accidental coincidences, and for those invisible minute changes in the internal parts of the body, which have been called metaphorically, the efforts of the constitution, and by observing carefully the sensible operation of any particular drug, and its influence upon the disease, in numerous and various instances, they learn its true virtues.”

“The latter on the contrary, consider that every alteration in the disease is owing to the immediately antecedent occurrence, and consequently they attribute, most commonly, the cure of any protracted or dangerous illness to the last medicine which had been exhibited, without taking at all into account the influence of the means previously employed, or the progressive operations of the system which naturally happen in the individual disorder. It

might, however, be expected, that both the scientific and uninformed observer could draw no other than one inference, in every instance in which the disease, from experience, is found to be *incurable*."

"Obvious, and even trite, as these remarks may appear, they cannot be misplaced, if they lead the profession to adopt a consistent mode of practice in the disease under consideration."

"All the phenomena, both of the acute and symptomatic hydrocephalus, imitate so precisely the symptoms which arise from a mechanical injury of the head, followed by inflammation of the membranes of the brain, that it is quite surprising the coincidence has escaped the notice of practitioners. Thus the violent pain of the head exciting great irritation of the stomach and *primæ viæ*, and afterwards the convulsions, the delirium, the stupor, and the stertorous breathing—which may be said to be in general the only invariable symptoms in hydrocephalus—are precisely the same which occur in injuries of the head under the circumstances specified."

"Although the consideration be humiliating, it is too instructive to be passed over, that the very reason urged by practitioners of deserved eminence for the employment of mercury in hydrocephalus, are not unfrequently in direct contradiction to their own explanation of the nature of the disease. A late writer on this subject of high reputation, for example, attributes hydrocephalus acutus to increased action of the arteries, with at the same time venous congestions within the cranium, and recommends mercury for the purpose of substituting a new action. But the most ample proofs, it is presumed, have been brought forward in the preceding pages, to show that mercury increases the arterial action, that it alters the nature of the circulating mass, and that it impairs the energy of the nervous system—and therefore its operation must tend directly to aggravate those alleged causes."

"On this subject the author can express his sentiments with more than usual confidence, having from his earliest years had innumerable opportunities of attending to the effects of mercury in this disease. In no instance under his observation has that medicine ever proved successful, and he fully agrees with Dr. Blackall in opinion, that in many cases the injudicious use of that mineral has actually occasioned the disease."

On the subject of ovarian dropsy he thus expresses himself—

"Upon what principle mercury has been so universally employed in all cases of enlarged ovary, notwithstanding the variety

of age, constitution and state of general health of the individuals affected with it, no satisfactory explanation has hitherto been given."

"Analogical reasoning is little in favor of the practice. Hydatids have never been cured by mercury, even when situated in parts of the body where that mineral could exert a ready influence. As to accumulations within the proper coat of the ovary, they are too isolated and unconnected with the absorbent system, to be affected by medicines capable of increasing the actions of the lymphatics."

"Far less can experience be pleaded in justification of this practice, for the author speaks within bounds, when he avers, that he has known mercury employed in some hundred cases of diseased ovarium, without its having proved useful in a single instance. A few apparent exceptions have been reported to him by old pupils; but from the uniform result of all the cases which have been under his own notice, he is induced to believe, that in those alleged exceptions, the disease had not been enlargement of the ovary."

"But while mercury can be of no utility whatever, it may, and certainly often has produced irreparable injury, not only upon the general constitution, (for the reasons so fully explained in this work,) but also in all cases where indurations of portions of the ovary are complicated with serous collection. Instances of this kind every now and then occur, and cannot be distinguished till their progress towards cancerous ulceration be so far advanced as to become manifest. A most impartial attention to many of those cases has convinced the author, that indurations, which might have remained for years without inconvenience to the patient, have been forced into morbid activity by a course of mercury."

"From the inutility or the injurious tendency of the various means commonly employed in cases of enlarged ovarium, the author for many years confined his views in the treatment of that disease, to promoting the general health, and to palliating distressing symptoms; and as he not unfrequently saw instances where the local affection, after a certain progress became stationary, and ceased to give any uneasiness, he supposed that no other resource could be safely relied on."

Of the use of mercury in Croup and inflammation of the Iris.—Perhaps there is no one disease in which mercury has been more generally administered, and upon the treatment of which, physicians of this country have been more unani-

mous, than the Croup. The frequency of its occurrence, the rapidity of its march, and the fatal tendency of its symptoms, have all conspired to render it a subject of more than usual solicitude; it therefore has received a more than ordinary degree of attention, and may be considered to be as well understood as any complaint to which infancy and extreme youth are liable. Bloodletting, antimonials, and calomel, one or other, or all, are the remedial means which are not to be dispensed with, however varied the prescriptions may be, in other respects. It therefore created some surprise, to find Dr. Hamilton rather disposed to undervalue the mercurial practice, notwithstanding the success with which it is known to be attended; and to prefer trusting its cure to the more usual remedies exhibited to subdue inflammatory action, such as bleeding, blistering, purgatives, and antimonials, to the exclusion of that medicine, which has not inaptly in this disease, been denominated the sheet-anchor of the physician's hope. The following remarks will enable the reader to judge of Dr. H's opinion of the remedy:

"About sixteen years ago, however, in consequence partly of the urgent representations of an old pupil (the late Dr. J. Anderson,) and chiefly because the ordinary remedies frequently proved uncertain, he was unduced to give calomel a fair trial—and he must say that the result of his experience has been so very different from that of the American practitioners, that he can explain it only on the supposition, that in different climates the same disease is relieved in different ways. At any rate he can solemnly assert, that according to all that he has seen, no relief whatever has been afforded by that medicine, unless copious dark green-coloured stools, like boiled spinage, have been discharged, and that it requires large and repeated doses of the medicine to produce that effect. For example, to a child of seven years old, one hundred and thirty-three grains were given within sixty hours."

"These circumstances led to the conclusion, that in this part of the world at least, wherever calomel has seemed to cure croup without affecting the bowels, the symptoms had not been those of the disease, but of the *spurious croup*; and this opinion is confirmed by the fact, that in the only cases in which the medicine has failed under the author's direction (being in the proportion of four

out of fifty,) no evacuation whatever through the bowels could be produced, although antimonial, and jalap, and gamboge, and glysters, were employed as auxiliaries."

"In reasoning upon this subject, it is extremely difficult to explain, in the first place the safety with which a hundred and thirty-three grains of calomel could be given in this climate, within sixty hours, to a patient of seven years of age; and, secondly, the relief which has invariably followed the discharge of the dark coloured evacuation. This latter circumstance, in particular, is the more perplexing, seeing that in all those cases the evacuation from the bowels were not *fæces*; but were combinations of bile and mucus in a substantial form, resembling, as already stated, boiled spinage, or the contents of the second stomach in ruminating animals."

"Topical inflammation, it has been long known, may be relieved by watery, or as they have been called, chylous discharges from the bowels, and their efficacy in that respect has been usually explained, on the supposition, that a considerable portion of the circulating mass is in that way thrown off, through the exhalents and other minute vessels of the surface of the alimentary canal."

"But the immediate operation of calomel, where successful in croup, is most obviously upon the Liver, and yet there is no reason to suppose so direct a sympathy of the Larynx with that organ, as with the other viscera concerned in digestion. It would lead, however, to discussions totally inconsistent with the purpose of this work, to pursue this inquiry further, though it certainly merits the most minute investigation."

"However inexplicable the *modus operandi* of calomel, in cases of croup, appears to be, the success of the practice was such, as to encourage the expectation of its proving a safe and certain cure for that formidable disease; though, while he indulged this hope, the author, even from the beginning, was not inattentive to the probable dangers which might arise from the indiscreet employment of so active a medicine."

"Thus he stated expressly, that "In some cases considerable weakness remains after the crouping has ceased, in consequence, partly of the violence of the symptoms, and partly of the operation of the calomel," he added, that he had seen "two cases, where, although all symptoms of the croup were removed by the use of calomel, the patients sunk from the weakness which followed;" and concluded, that "those cases enforce the necessity for carefully watching the progress of the disease, so as to stop the calomel whenever the symptoms begin to yield."

"Since that publication, he has been favoured with numerous communications on the subject from old pupils, settled in distant parts of the kingdom, which confirm his former account, that the disease occurs in various degrees of violence in different situations,

and which convince him, that the severe forms of this affection do not yield to the exclusive use of calomel. He has suggested, therefore in such cases, that the chief reliance should be put in the remedies more usually employed to subdue inflammatory affections, viz. bleeding, blisters, antimonials, and brisk purgatives. In giving this advice, he has commonly taken the liberty to add minute directions for the employment of those means, being aware, that in every acute disease, the progress of which is so rapid, every thing depends upon the timeous and steady application of remedies."

With respect to the acknowledged success of Mr. Saunders and Dr. Farre, in the cure of *ophthalmia iridis*, by means of mercury, Dr. Hamilton observes, that it is most probable the influence of the calomel was restricted in its beneficial effects to those cases in which the syphilitic virus required to be subdued before the cure could be completed, and was not as extensive in its operation as Dr. Farre would infer. To the opinion of the latter gentleman, that "mercury alters the action of inflamed arteries, more especially in respect to the effusion of coagulable lymph, which it in varied degrees controls, or even altogether suspends," he opposes the following facts :

"It has been shown, that the action of mercury tends, by exciting inflammation and effusion, to produce thickening of various membranes particularly of the pleura, and several instances of that kind are recorded by Dr. Blackall, where the fact was proved by the appearances on dissection. This effect of mercury has long been remarked by the author, and it suggested to him, many years ago, a practical inference of some importance."

"Every surgeon knows, that in cases of abscess of the mamma in the puerperal state, sinuses are apt to form, which are little disposed to heal, and which discharge a thin sanies. It is acknowledged, that the operation for the fistula ani is sometimes followed by a flabby pallid state of the parts, a tendency of the sinus to elongation, and an ichorous discharge. In many instances of both these affections, the author has advised the use of the blue pill, so as very slightly to affect the gums ; and he has generally found, whenever that event took place, the granulations became healthy, a well conditioned discharge appeared, and the healing process advanced rapidly. These facts are quite inconsistent with the hypothesis, that Mercury suspends the effusion of coagulable lymph."

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“ The Author intended originally to have considered the utility of mercurial medicines in Glandular Affections, in Chronic Rheumatism, in Chronic Eruptions of the Skin, and in Anomalous Complaints, but these pages have extended so much beyond his expectation, that he feels it necessary to conclude. He does this the more readily, because the remarks he has offered upon the effects of Mercury on the arterial system, and on the condition of the circulating mass, may serve to illustrate the cases where that Medicine can be useful or prejudicial.”

“ Particular combinations of Mercurial Oxydes with other medicines, may, he is well aware, be employed advantageously on various occasions, and accordingly he believes that in certain stages of Glandular Affections, of Chronic Rheumatism, and of Chronic Eruptions of the Skin, the addition of a very small proportion of the Muriate of Mercury to some of the compounds of Antimony, increases the activity, without lessening the safety of those latter medicines. But he has always found, that when Mercury in any form is employed as an auxiliary, it can scarcely be given in too small doses ; and at any rate, he ventures to advise, that when so administered, it ought never be allowed to produce its proper sensible effects upon the system.”

“ He takes the liberty to add, that in the preceding pages, (as has been repeatedly stated,) his observations apply exclusively to the Use and Abuse of Mercury in this part of the world. Although through the favours of a most intelligent Medical Friend, he has had access to some official documents, which convince him that Calomel has been injudiciously employed in the remittent fever of the West Indies, he has purposely refrained from making any animadversions upon that practice.”

The foregoing extracts will enable the reader to form a tolerably correct idea of the merits of Dr. Hamilton's work. We have said above, that it was a book of no common character, and that its tendency cannot fail to be useful, and in that opinion we are confirmed, not so much by the value of his facts and the correctness of his reasonings, as by the general attention which it will be the means of directing, to the interesting subject to which it is devoted. Mercurial medicines are articles of habitual prescription by almost every class of physicians, and nothing is better calculated to elicit a spirit of inquiry into the propriety of their use, than an ingenious and well directed attack against their effi-

ciency, and in opposing the remedy in such general terms, and to so great extent as Dr. H. does, he must necessarily impugn the discretion, if not the intelligence, of a large portion of the medical profession, who in the present state of the science, will not tacitly acquiesce in the censure, without thoroughly sifting the facts and arguments on which it rests.

It cannot have escaped the observation of the reader, that our author has singled out as points of support for his opinions, those very diseases, in which heretofore mercury has been most extensively used; the inquiry therefore becomes the more interesting, inasmuch as it involves a question of the propriety of practice in those cases wherein physicians have pretty generally agreed. If the work did not commend itself to the careful perusal of every medical reader, we should feel it to be our duty, however it might suit our inclination, to criticise its doctrines; as it is, we are absolved from that necessity, though we cannot help remarking, that we think Dr. H. in many instances has drawn general inferences, from data much too narrow to permit them to be considered conclusive.

This remark is particularly applicable to the fifth, sixth and ninth sections of his book. The experience of every practical writer on the diseases of warm climates, where febrile disease almost invariably involves the liver, bears ample testimony not only to the propriety, but necessity of mercurial medicines, and the success attending its exhibition affords the most satisfactory answer to all those scruples which timidity may indulge, while brooding over the possible evils which may flow from idiosyncracies of constitution. In chronic affections of the liver, no medicine hitherto discovered, can with mercury bear a comparison, and although it be true that ill effects may, and often do result from it; it must be remembered that the universality of its use could never have arisen from any other cause than the known success

attending the practice. Even in cases of induration accompanied by a deranged state of the chylopoietic viscera from continuance of disease; the want of success in treatment where it may be, and very frequently is unsuccessful, is no more chargeable to the remedy, than are the unfortunate results of inflammatory diseases to their several plans of treatment, although they are all well settled. The truth will be found to be, that it is not the mercurial medicine which deserves reprobation, so much as that deficiency in physiology and general pathology on the part of the physician, which permits him to overlook, or heedlessly to neglect the varied changes which from day to day occur during the operation of general remedies.

In conclusion we shall only observe that if ever Dr. H's book should without a thorough examination be considered as a standard of authority, it would do much to spread the contagion of *Hydrarphobia*, which we already perceive exists sporadically in some of our colleges, and occasionally threatens to be a serious source of future evil to the junior members of the profession.

The notes which the editor, Dr. Ansel W. Ives, of this city has interspersed through the work, for the most part illustrative of some of the positions of the author, are individually very interesting, though we think that they are neither so numerous, nor so argumentative as the text would warrant; but as it could not have been his intention to controvert Dr. Hamilton's opinions, he certainly is not answerable for this deficiency.

The appendix entitled *observations on the modus operandi of mercurial medicines, and on their use in febrile diseases*, is precisely what its title imports, and this by the way is no slender commendation, for it is not usual in these days to find an author or an essayist confining himself to the subject which he proposes to treat, to the exclusion of all other matters. It is a neat, concise and judicious exposition of its

author's views of the operation of mercury, and an attempt to deduce some general principles which may direct its exhibition in those diseases which are accompanied by fever.

A Reply to "Remarks on Certain Parts of an account of the Yellow, or Malignant Fever, as it occurred in the city of Philadelphia in 1820;" and some observations on "A Reply to the Remarks of J. on the Review of the Papers relating to the Fever in New-York in 1820," which were published in the 4th vol. American Medical Recorder, October, 1821. By SAMUEL JACKSON, M. D. Philadelphia.

The American Medical Recorder, for last October, contains two papers, in which I am assailed with a degree of rancour, that must appear strange, and be wholly unaccountable to those unacquainted with the feuds which have heretofore distracted the Medical Profession the city of New-York.

The observations that excited these bursts of passion, and that seem to have touched more deeply than was conceived possible, or even contemplated, were not intended to abet the designs of a party, or injure any individual. Their aim was much higher. Truth was their object; and in its pursuit, authorities wrongly quoted, and statements erroneous and unfounded, were unhesitatingly exposed, perfectly independent of any sentiment of personal animosity, or purpose to inflict a personal annoyance. If individual feelings have suffered, the circumstance should be imputed to its proper cause. Every medical disquisition should be conducted in that sober and unbiassed temper, that can alone render it of utility, or calculated to accomplish its only legitimate purpose—the establishment of truth. When it assumes a different character, and, more especially, when, from levity, rashness, prepossession, or other motives, a departure from the most

rigid observance of accuracy and correctness, in assertions and positions, is hazarded, it becomes a solemn public duty, that the delinquency should be pointed out, and animadverted on by some one. Preparing to submit my remarks on the malignant fever of 1820 to the public, although not as well qualified as many others to the task, yet it was not irrelevant to notice the inaccuracies of the "Discourse on Medical Police." This is the offence, that has awakened so much ire; and procured for my humble productions the distinction of having given rise to such passionate criticisms.

From a note, attached to one of the papers, to which I have alluded, Dr. Eberle, one of editors of the Recorder appears to have been conscious, that the personal allusions contained in that paper, are calculated to disgrace its author. To remove from himself every suspicion, that he could be guilty of the improprieties it contains, he disavows any participation in them; and indicates Dr. Ducachet, his co-editor, as the writer, to whom belonged, this evomition of venomd spleen.

The indecent personalities of this writer, are to be treated with contempt. Reputation for abusive writing, is cheaply obtained, by those, to whom it is an object of ambition. I have no desire to detract a particle from that, which Dr. Ducachet and his co-adjutant writer, have laboured so earnestly to merit, and of which they have shown themselves so worthy;—Let them enjoy it in the fullest fruition.

I disclaim the slightest disposition to lessen the just reputation of Dr. Hosack, or any other individual; but I have to learn, and cannot imagine, what are the circumstances that so elevate him above common mortals, that to question his infallibility, or examine his literary productions, is to be accounted a presumption, deserving to be overwhelmed with contumely and reproach. Dr. Ducachet should have reflected, that it was possible, the "distinguished man," who filled, in his imagination, so vast a space, might hold but

a moderate standing in the estimation of others. The beetle that circles with drowsy hum around the evening taper, dazzled with its glare, may imagine it a glorious luminary in the little world in which it exists. Though the declaration may appear inordinately arrogant to Dr. Ducachet, I can assure him, I have no apprehensions of being esteemed egregiously vain, should I engage in any contest with this "god of his idolatry."

"Victorque virū volitare per ora."

My thanks are due for the quotation, and find in the triumph, but little to satisfy the cravings of a very moderate ambition.

Dr. Ducachet has taken the liberty to imagine my motives for the heinous crime of questioning Dr. Hosack's professional accuracy. He can take no offence, should I imitate his example.

Honest old Burton, with no little indignation, describes the company of parasites and flatterers, who, with immoderate praise, bombastic epithets, glossing titles, and false eulogium, so bedaub, applaud, and gild over, many an undeserving man, that they clap him quite out of his wits. Their hero is never in his proper character, but always appears in masquerade. "If he be a big man," says our quaint author, "then he is a Sampson, a Hercules; if he pronounce a speech, another Tully, a Demosthenes; if he can make verses a Homer, a Virgil;" and should any one laugh at these ostreperous claims, he is immediately, "a Zoilus carping at Homer's genius." We are here presented with a picture, for which a prototype might be found. But having somewhat more charity than the wrathful doctor, no use will be made of it; and his example and his motives, be permitted to rest with himself.

Dr. Hosack, I feel assured, has too much good sense to countenance these ridiculous pretensions that are made for him, or to quarrel with those who reject them. It might,

otherwise, be suspected, he had been "clapt out of his wits," and had adopted the vain boast of Ennius—

"Volito docta per ora virum."

The papers to which I have alluded, would have been earlier noticed, but for the engagements in which I had entangled myself at the period of their publication, and which occupied almost exclusively, my attention and time. During the winter I was employed in delivering a first course of lectures in the college of Apothecaries, which was prepared from day to day; and, consequently, enjoyed but few moments of leisure—I felt, besides, probably too indifferent on the subject. The inconsistencies, the sophistry, the unfair deductions of those writers are so glaring, that it appeared to me, they must strike the most casual reader. But I may be in error: few examine with attention, the matters contained in essays of the character of those published in the Recorder of last October, to which I have reference. Their allegations are too often admitted, especially if urged with boldness. It is therefore a duty, that I should substantiate all that I before advanced, and which these writers have denied; and repel the personal attacks, in which they have so freely indulged.

At my first leisure, I accordingly prepared an answer, intending to publish it in the Recorder. A suspicion never entered my mind, that I could possibly be denied the exercise of the same latitude of remark, and freedom of observation, provided I thought proper to use it, that had been employed towards myself, especially by one of its editors. It is obvious, that it is nearly impossible to rebuff arguments and allegations, intimately woven with rudeness and incivility, without the "retort courteous." Considering the provocation that had been given, my right, I conceive, was used with moderation. It is unpleasant, it must be admitted, for an editor to give publicity to his own scandal; but he should

take care not to place himself in the situation to deserve the exposure, or, by a refusal, to act with injustice.

Doctor Eberle, the editor of the Recorder, residing in this city, received my communication, intended for the April number, and put it to press. The proof was sent to his co-editor, Dr. Ducachet in New-York, the writer of the essay to which it was an answer. The proof was returned by him, with several erasures. The rejoinders to Dr. Ducachet's personal remarks, appeared to have been peculiarly offensive, as none escaped his censorial excision. As my communication was placed under editorial interdict, unless I submitted to this bare-faced injustice, it was withdrawn, rather than have it published in a mutilated condition. The editors of the New-York Medical Repository, immediately granted my request, for the admission of my replication on the pages of their valuable journal, and I am thus enabled, by their politeness, to defeat the littleness of the policy, that has dictated this unmanly conduct. The reasons that Dr. Ducachet may have assigned, for rejecting the passages on which he exercised his censorship, have not been made known to me. They could not have been, most certainly, an objection to the publication of personal allusions. The Recorder has not been remarked for its purity in this respect; and they would come with too ill a grace from so notorious an offender as the New-York editor of the Recorder, to be tolerated.

A plain relation of the circumstances connected with this controversy, will more strongly picture the conduct of Dr. Ducachet, and elucidate its character, than any commentaries I can offer on it.

In the Recorder for April 1821, a review was published of the report of the New-York Medical Society, on the *Bancker-street fever*, to which was attached the initial letter B. The committee of the Medical Society, some of the most intelligent and distinguished of the profession in New-York, was

treated in this paper, with little ceremony and less delicacy. The subsequent number for July, contained a paper signed J. devoted to the discussion of a single point which had been made in the report of the Medical Society, was contested by the reviewer B, the correct determination of which, is of considerable interest and importance in a pathological view. The writer of B, was well known; but, as he chose not to declare himself openly, although under his disguise he did not spare others, his wish for privacy was respected. No personal allusion was made to him by J, whose remarks were confined to the subject-matter under examination and growing out of the review of B.

This paper called forth a reply, filled with petulant and angry invectives directed to me, as the author of the remarks of J. Dr. Eberle, as was mentioned before, seemed to feel so sensibly the indecency of this unprovoked attack, that he thought it necessary to disavow a participation in the personal allusions, and designated Dr. Ducachet, his brother editor, as the writer of the anonymous paper that contained them. Dr. Ducachet has, thus, in this first place, infringed that comity which every writer who respects himself in the opinions of others, will observe in a discussion of a scientific topic, by mingling with it personal reflections on an opposing writer; this offence becomes peculiarly flagrant when the discussion is anonymous. In the second place, he has violated the confidence reposed in him as an editor, by a correspondent, without the slightest provocation to justify the outrage. To point the climax that marks his conduct, he takes advantage of his editorial privilege to close his journal to the replication to his own attacks. I wish it to be understood, that it is not intended to object to, or to complain of the personal reflections contained in the papers the subject of these observations; of the editors of the Recorder, favours were neither expected nor asked: simple justice alone was looked for—that my right to reply through the

same medium to the aggression I had received, should not be opposed ; but in this reasonable expectation I have been deceived. The object of this short narrative, is merely to display the principle of Dr. Ducachet, and to explain the cause of my answer appearing in another journal, than the Recorder.

These prefatory observations have occupied a greater extent than was intended. I shall now, without further prelude, proceed to the consideration of the papers in question, examine into their character, expose the means both of defence and attack, that are employed in them ; and exhibit the just value of their pretensions to correctness, research, and information.

The first paper purports to be "Remarks on certain parts of an account of the Yellow or Malignant fever, &c." Its object is to defend the accuracy of Dr. Hosack's references and quotations, in his "Discourse on Medical Police," which I had impeached ; and to retort on myself, the allegations I had made against the late "Resident Physician for the city of New-York, Professor, &c."

As it regards the character and kind of defence, attempted in this paper, it is to be remarked, that the inconsistencies I pointed out in the "Discourse, &c." are unnoticed, and, consequently, are admitted to be accurate. Of the instances of "deceptive references, to authors for opinions, on subjects they do not treat, and for facts they do not mention," that were indicated ; while an effort is made to contest some of my charges, others are not controverted, and are of course also admitted to be true. Thus it results, that it is allowed, by the champion of the "Discourse on Medical Police," that Dr. Hosack has been inconsistent with his principles ; and has made some deceptive references. His defence then amounts simply to this : some of the references are not deceptions.

Had this writer confined himself to the proof of this position, so far as it could be yielded to him, by a candid examination of the authorities disputed, and a correct report of the passages in controversy; I should not have felt myself called upon to write another line on the subject. But he has acted otherwise; he has charged me with being guilty "of the very practices against which I inveighed." He has made it obligatory on me to establish the truth of my allegations, the perfect correctness of my quotations, the candour and justness of my deductions.

If, in doing this, the defence that has been attempted of the poor remnant of the "Discourse on Medical Police," and the puny endeavour at revenge, is shown most distinctly to be a mere mass of misrepresentation, quibbling and ignorance, though it may appear harsh, yet it is a proceeding that has been rendered, and I regret it is so, entirely unavoidable.

That this description is not overcharged, but true in every respect, will become manifest, in the detailed examination of the particular points contained in the "Remarks on certain parts of an account, &c." to which I now pass on.

The appeal made in the "Discourse on Medical Police," at pages 8 and 9 to the names of Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, Blane, Chisholm, M'Gregor, Pym, Gilpin and Wright, in a manner to impress those not conversant with the writings of these authors, that they had been all perfectly familiar with Yellow Fever, treated of it in their works, and inculcated its contagion; I adduced as an instance of deceptive reference. What is the defence? I am accused of "misrepresenting the language of the Discourse on Medical Police." It is positively asserted that Dr. Hosack "is not speaking of Yellow Fever, but of contagion as appertaining to fevers in general." To sustain this assertion, part of the passage on which I animadverted, is quoted from the Medical Police. The fair admission of this defence is, that if Dr. Hosack is speaking of Yellow Fever,

when he refers to the distinguished physicians whose names are mentioned above, then is the reference deceptive. I will now show that he is speaking of Yellow Fever, as well as "of contagion as appertaining to fevers in general," notwithstanding the very positive denial, that is made to the contrary. I shall do this, by continuing the quotation of the writer of the "Remarks, &c." from the part at which he found it so very convenient to his purpose to close abruptly with an &c. This very handy &c. happens to cover the only part of the passage on which I animadverted, and in which Dr. Hosack is "speaking of Yellow Fever.

"Are the investigations and the accumulated experience of Huxham, Haygarth, Currie, Gregory, Ferriar, Percival, Blane, Chisholm, M'Gregor, Pym, Gilpin, Wright, and a host of others, to be prostrated by the arrogant assertions, the overweening conceits, and flippant remarks of those juniors in knowledge and in years, who have lately obtruded themselves upon the public attention? Although they do not merit a serious and laboured refutation of their mistaken views, it will be at least proper, under the various points which they consider at issue, to call their attention to those important facts, and those sources of information, with which they appear to be unacquainted, or which, in their eagerness to promulgate their effusions, they have totally disregarded. *With this view, I shall notice their lucubrations under the several heads in which they dissent from the truths that appear to have been established by the experience and observation of the distinguished physicians to whom I have already referred, (viz. Huxham, &c.)*

In the first place, they deny the peculiar character of yellow fever as distinct from the ordinary bilious and typhus fevers of our country."

This plain language admits of no misconstruction; Doctor Hosack *does* speak of yellow fever, in connexion with his references to the names of the "distinguished physicians," and it follows, that by the mouth of his defender, is his condemnation pronounced.

I do not hesitate to confess, that I was led astray by the loose, unphilosophical, and disorderly method of quoting authorities, as a cover to every sort of license which a writer

may choose to exercise with them, pursued in the Discourse of Medical Police, and mistook the Dr. Wright to whom reference was made. I inquired of several friends to ascertain who was this Dr. Wright, but ineffectually. I ransacked several extensive Medical Libraries for his works, but to no purpose. The truth is, it never, for a moment, occurred to me, that the writer of a few essays in a periodical work, would be associated with some of the standard authorities* of the science, without a special reference to the work, containing the paper on which the appeal was authorised.

If it can afford to Dr. Hosack and his defender, the slightest satisfaction, to convict me of ignorance of Dr. Wright and his essays, I freely afford it to them. I plead guilty.—I will go further.—I put in the same plea with respect to some hundred other essayists, in the thousand volumes of Medical Periodical Literature, which the spirit of the age threatens to render interminable, of whose names and writings I have no knowledge.

The able defender of the Medical Police gravely informs his friends, that “Dr. William Wright of Jamaica, (why could not Dr. Hosack have been as particular), is known to every student of medicine, who reads his dispensatory, for his description of the *Cinchona Caribæa* and *Geoffrœa Inermis*.” If I understand the meaning intended to be conveyed by this observation, it is, that as Dr. Wright had given a description of the *Cinchona Caribæa* and *Geoffrœa Inermis*, he should have been known as an author on Yellow Fever. This is most admirable logic. The sequitur, an induction that admits of no refutation. I feel myself incapable of answering such an argument, but must cry mercy, and quietly submit to this irresistible proof, that I am an ignoramus!

The writer of the “Remarks, &c.” has placed beyond a doubt, the propriety of Dr. Hosack’s reference to Dr. William Wright, as a writer on Yellow Fever. But, in establishing this point, he contradicts his own positive assertion,

and destroys his own argument. He had just asseverated most peremptorily, as it then suited his object, that "Dr. Hosack does not assert, even by implication, that any of the writers referred to (Dr. William Wright one,) had seen yellow fever or treated of it in their writings. He is not speaking of yellow fever." Yet, in the succeeding paragraph, he furnishes the proof that Dr. H. is "speaking of yellow fever," by adducing the evidence, that in referring to Dr. Wright as authority in yellow fever, Dr. H. could not be charged with a "deceptive reference." Thus, like an unskilful engineer, whose works, disposed without judgment or system, batter down each other, this writer refutes his own arguments, and overturns his own statements.

While on this point, on which this "able writer," as he is styled by Mr. Coleman, (a profound judge) has exhausted his wit; there is one observation, intended to be so very sarcastic, I would not hurt his self-love so much, as to let him suppose it had been unfelt, by leaving it unnoticed.

Dr. Wright, I mentioned, had written a history of the Walcheren Remittent. A few pages preceding, I had asserted, that the writings of the authors quoted by Dr. Hosack (a Dr. Wright one of them), "are confined to diseases observed in England. It was supposed I had here committed a lapsus, that is pounced on with all the voracity of a vulture ravening its prey, in the expectation of finding food for malice.

"Without pretending," observes the writer of the remarks, in the least to call in question, his knowledge of geography, I would beg the Philadelphia President (biting sarcasm) to inform me, whether the Walcheren fever (remittent) occurred in England? This question, I by no means consider "impertinent," of which he expresses an apprehension. It has too much of simpleness in it, for so harsh an appellation; it is best answered by asking another. Does not Dr. Hosack contend, that yellow fever is an inter-tropical or West India

fever, and cannot be generated in New-York ; and does not this inquirer know, that Dr. Hosack has written on this West India fever as it occurred in New-York ? There is no difficulty to prevent the Walcheren remittent, from occurring and being observed in England. A few hours sail brings those whose systems are imbued with the exhalations of the Walcheren marshes into Great Britain, where the disease subsequently unfolds itself. This is not a hypothetical occurrence, but a well established fact.

Few medical readers have now to be informed, that the Walcheren fever occurred extensively in England, on the occasion of the memorable and disastrous operation of the English forces against Zeeland. The following passage from Dr. Wright's History of the Walcheren Remittent, a work composed from the observation of the disease in Harwich Hospital, and not in Walcheren, is one, amongst numerous proofs that could be adduced. "For, healthy regiments, (as I have it from Dr. Patrick, an authority I must ever respect,) brought over without sickness from Walcheren, and quartered in Colchester, were universally seized with the continued remittent of Walcheren in its characteristic form, and the sequel was the same as at Walcheren and Harwich, so that no proof was wanting to ascertain their identity."* I hope this inquiring gentleman's "anxiety to become intimately acquainted with my novel views" on this subject, is now gratified.

As this gentleman professes an ardent desire to be made acquainted with novelties in geography as well as medicine, he may gratify his curiosity, by turning to page 19 of Dr. Hosack's Discourse, where he will find the novel information, that Naples and Palermo are inland towns. I should not have thought it necessary to have noticed this "novel view," but for the "exceeding anxiety he expresses to in-

* History of Walcheren Remittent, page 19.

form himself on such subjects," and the pleasure it gives me to enable him to indulge such very laudable desires.*

Having pointed out the deceptive reference of Dr. Hosack to Huxham, as authority on yellow fever, I selected a few quotations of the general principles of that most sound and accurate observer, to show their hostility to the views entertained by Dr. Hosack. The force and point of these quotations, are incontrovertible. They are all admitted, except one, to be the very reverse of the doctrines of Dr. Hosack. Yet from this single passage would the defender of the Medical Police boldly account for the conformity of Dr. Hosack's principles with those of Huxham, and the propriety of his reference to this great authority. But the doctrine contained in the passage I cited from Huxham, and which is more largely extracted by the writer of the remarks, has no resemblance to the opinions of Dr. Hosack. Huxham, in union with Hippocrates, ascribes to the depraved constitution of the air, the *causes* of most epidemic diseases. The influence of this constitution is so great, (it is the opinion of Huxham,) as to affect "the increase and duration, but not as in other epidemics, originate such as are properly stiled contagious." This is the doctrine of Huxham, that the writer of the "remarks" avers, is precisely the same as "the views of Dr. Hosack concerning the contagiousness of yellow fever." I am sure Dr. Hosack must have felt surprised at this discovery.

There is not a syllable in the writings of Dr. Hosack, in reference to the influence of "atmospheric constitutions," on the character and prevalence of epidemic diseases. "The views of Dr. Hosack on the contagiousness of yellow fever," are evidently different. An atmosphere vitiated by the de-

* This passage of the Discourse on Medical Police, is taken nearly verbatim from Blane's Elements of Medical Logic, page 150, though it is not acknowledged. The blunder, however, is Dr. Hosack's, and not Sir Gilbert's.

composition of vegetable and animal matter, he teaches, "lends wings to the emanations proceeding from the diseased body."* But has this puerile notion the slightest resemblance to the philosophic doctrine of "atmospheric constitutions," as illustrated by Hippocrates, Sydenham, Huxham, and Stoll? Who can form any definite and precise ideas from terms so vague and unphilosophical? Examine the expressions in every possible manner, and it will be found, that they really mean nothing.

Dr. Hosack, in the most positive terms, adduced Pringle as having "abundantly drawn the distinguishing characters of bilious and yellow fevers. I proved beyond the possibility of cavil or denial, that Pringle was a zealous opponent of the Professor's doctrine, and endeavoured, not to distinguish between, but to identify "the characters of bilious and yellow fevers." Here was another open, manifest, undeniable conviction on my charge of a deceptive reference. What course does Dr. Hosack's champion take in this instance? Does he, like an honest and candid inquirer in the great cause of truth, acknowledge and lament the error of his hero? No; like a true Swiss, he knows no other cause than that which employs him. He seeks to screen the author of the *Medical Police*, from this most palpable misrepresentation, by accusing me "of sophistical reasoning," in my observations on the reference to Lempriere. The object of Dr. Hosack in establishing the distinguishing characters of bilious and yellow fevers, was to give support to his favourite theory of the contagiousness of yellow fever. This was the sole intention of the *Discourse on Medical Police*. In quoting Lempriere, therefore, as an authority to prove the distinct characters of the two diseases, it was Dr. Hosack's duty, as a candid writer to acknowledge, that Lempriere did not consider contagion as a diagnostic of either.

* *Discourse on Medical Police*, page 26.

Although no positive assertion is made in the discourse, that Lempriere believed in the contagiousness of the disease, he calls the continued endemic or yellow fever; yet, the manner, in which he is quoted by Dr. Hosack, leads unavoidably to that inference. The context admits no other conclusion. It was to guard against this source of error, arising from the culpable omission by Dr. Hosack of an important circumstance, that I mentioned the particular opinion of Lempriere, so little calculated to support the theory, into the service of which it had been pressed. How is it possible, that the most captious, having the smallest regard to truth, can convert a plain proceeding like this, into sophistical reasoning? But, admitting, for the sake of argument, that my statement of Lempriere's opinion, was sophistical reasoning, by what "process of critical legerdemain," can this juggler cause it to make the positive proof of the deceptive reference to Pringle, "a baseless fabric of a vision?"

One of the most glaring instances of a deceptive reference to writings, for facts and opinions that are not contained in them, is the appeal made by Dr. Hosack to the diseases of Seamen, by Sir Gilbert Blane. I repeat, that not a single fact or observation, is to be found in that very excellent work, to justify the reference made to it. The defender of Dr. Hosack, has the hardihood to assert, "that the whole of this is nothing more than the creation of Dr. Jackson's heated imagination." He avers, "that Sir Gilbert Blane in this very work, relates one of the strongest facts ever adduced in support of the contagiousness of yellow fever;" "it is contained," he continues, "in a letter addressed to the Hon. Rufus King, then minister at the court of St. James." Now mark the frontless imposition, that is here attempted to be played on the reader. The work on the Diseases of Seamen was published in 1787; the letter addressed to Rufus King, is dated 1793; a difference of eleven years in the period of the publication of the two productions; besides, the

facts related in that letter, were only reported to Sir Gilbert, and are not his own. If Dr. Hosack's reference was to the letter to Rufus King, why did he not so state it? why did he particularize "the facts adduced in the diseases of Seamen?" Did it arise from his knowing that the relation of facts contained in that letter, had been invalidated, by the persevering researches of Dr. Bancroft? The letter to Rufus King may very probably be added, as an appendix to subsequent editions of the Diseases of Seamen, but this will not authorize a reference to the body of the work. The edition in my possession is the first, and I again reiterate, that there is not the slightest allusion to any occurrence, observed by Sir Gilbert, in the numerous fleet of which he was Physician General, in the West Indies, that appears to have even awakened his suspicion, that the disease was contagious. He has not been, I believe, in the West Indies since; or had an opportunity to acquire any additional experience from personal observation.

In the Discourse on Medical Police, Dr. Hosack lays down, as a position of important bearing, "that until the affirmative testimony contained in the writings of Dr. Chisholm, Dr. Wright, Sir James M'Gregor, Dr. Pym, and Sir Joseph Gilpin, &c. shall be disproved, the negative declarations of the late writers to whom I have referred, must be discredited." Those who have not investigated the question at issue, must have supposed from Dr. Hosack's statement, that the "affirmative testimony of Dr. Chisholm," &c. had never been disproved. I instanced this position of Dr. Hosack, not so much as a deceptive reference, as an attempt to deceive the uninformed. The affirmative testimony of Dr. Chisholm, is the most important of that of all the contagionists; it may, in reality, be considered, as constituting the basis on which the whole system of the contagion of yellow fever rests. It is not surprising, therefore, that Dr. Hosack lays so much stress on it; but the refutation of this "affirmative testimony," has been so complete and entire, and has proceeded from so

many sources of the highest authority ; even from those, whom Dr. Chisholm had cited as witnesses, that not a loop is left on which to hang a doubt. No one, who has devoted a small portion of time to the investigation of this subject, can be ignorant of this well-known circumstance ; and it is incredible, that it should have been unknown to Dr. Hosack. How then, could he, as a conscientious investigator of the truth, cite the "affirmative testimony of Dr. Chisholm," as having never been disproved, when he could not but have been aware, that not a single point of it remained unquestioned or unrefuted ?

The apologist and advocate of Dr. Hosack, with his accustomed aberration from correctness, sounds his defence of this art of deception, by mistating Dr. Hosack's language and meaning. He asserts, that Dr. H. merely "adduced these writers, as having furnished affirmative testimony in favour of the contagiousness of yellow fever." This is an unfair representation of Dr. H's. assertion. The position taken by Dr. H. is, that "the affirmative testimony adduced" had never been "disproved ;" a point altogether different from that stated and defended by the advocate of the discourse on Medical Police. We might be led to suppose that the frequent mistatements of this writer, arose from an incapacity to comprehend the plainest language. Reflection must, however, convince, that such cannot be the case ; but, that the task he has undertaken is of so hopeless a character, no other course is left for him to pursue. With a generosity, that can scarcely be commended, though it is rare, he seems willing to share the fortune of his friend, and the conduct he can neither fairly justify nor defend, he will boldly imitate—

"Common the crime, then common be the pain."

In a virtuous cause, self devotion like this, constitutes the noblest character ; but, in a cause intrinsically bad, it only makes

"An artful manager, that creeps between
His friend and shame, to be a kind of screen."

But this writer is not willing to be considered as sophistical and erroneous only, he must be inconsistent and absurd. Dr. Chisholm's testimony on the contagion of yellow fever, consists principally of the romance of the ship *Hankey*, and the circumstances connected with her arrival at St. George's in Grenada ; all of which have been manifestly shown to be the mere coinage of his brain. Separate the story of the *Hankey* from Dr. Chisholm's proofs, of the contagion of yellow fever, and they become a nullity. The doctrine of the importation of yellow fever, from *Bulam* to Grenada in 1793, and thence to all the other West India Islands, and our continent, rests on the truth of this story. Destroy this foundation, and down goes the whole structure of contagion. Yet, this logical writer avers, that the overthrow of this groundwork of Dr. Chisholm's testimony, "is not fairly meeting it ; does not weaken or destroy its force ; is flying off from the subject matter of the dispute ; is mistaking one thing for another." This Quixotical mistake of mine, as he considers it, differs in one respect from the famous adventure of the renowned knight, to which it is facetiously compared : it produced more of anger than smiles, tears than laughter. Dr. Hosack and his friends, judging from the passion they are moved with, do not appear to consider it a joke. Nay, if Cervantes had made his hero guilty of no greater hallucinations, than such as taking Dr. Hosack for a wind-machine, instead of a giant, the world would have wanted many a hearty laugh, in the dull recital of sorry realities.

"The affirmative testimony" of Mr. Pym, which Dr. H. made of so much importance, as not having been disproved, is in the same predicament as that of Dr. Chisholm. I alleged, that part of it had been refuted by Dr. William Ferguson, and, as his system was built on the correctness of Dr. Chisholm's account of the origin of the *Bulam* fever, the testimony of both fell together. But, nearly the whole of the testimony of Dr. Pym, has been clearly and amply con-

troverted by Dr. Bancroft, in a late work, which my friend, Dr. Chervin, had in his possession, and which I hastily perused, while he was prosecuting his interesting and valuable researches in this city. I have not been able to meet with any other copy, or I might have it in my power to show what is the degree of credit to be attached to the Stoney Hill story. The advocate of Dr. Hosack, however, denies that "Mr. Pym builds his system upon the origin of yellow fever, on board the ship Hankey." I contend, that he does assign its origin, which involves the character of the disease, to its importation from Bulam by the Hankey, as related by Dr. Chisholm. Here we are it issue; let facts decide. The title alone of Mr. Pym's work, is sufficient to establish my position, and gives a flat contradiction to the assertion of my opponent. Mr. Pym's work is entitled "Observations on the Bulam fever, which has of late years prevailed in the West Indies, on the coast of America, at Gibraltar, and other parts of Spain." Does not this title page present an epitome of Dr. Chisholm's doctrine; which is, that the yellow fever was imported by the Hankey from Bulam, into the West Indies, whence it was carried to America, and communicated to Spain, &c. But Mr. Pym settles the question himself, by defining his meaning of Bulam fever, viz. "Bulam fever, a contagious disease of foreign origin, and supposed to have been imported from the coast of Africa," page 4. He is still more explicit. At page 199, the following observation will be found in a parenthesis; "without any reference to the introduction of the disease into Grenada, as mentioned by Dr. Chisholm." The extract of his letter to Sir Richard Keates leaves no doubt, as to the correctness of my assertion, and the unfounded, though bold declaration of my accuser: "In my opinion, the fever, which prevails on board the transports from Carthagera, is the contagious fever of the West Indies, known by the name of the Bulam fever, from its having been imported from that settlement to the island of

Grenada, in the year 1793." It is the same disease which prevailed in Spain in the years 1800 and 1803, and at Gibraltar in 1804," page 236.

Notwithstanding this plain and positive avowal of his belief by Dr. Pym, the defender of Dr. Hosack, who pretends to doubt, whether I had read Mr. Pym's work, and must of course be familiar with it himself, denies that Mr. Pym builds his system upon the story of Dr. Chisholm. Of whom else does he get the name of Bulam Fever? Of whom else does he derive its importation from Africa? That the opinions of Mr. Pym are precisely as I stated them; after this exposition, it might be supposed the most daring would hardly deny. Now, of what value is the "affirmative testimony" of Mr. Pym, who makes the disease, that has afflicted the different cities of our continent, Gibraltar, and various cities of Spain, to be derived from Bulam, where no such disease has been known; and to have been imported from that settlement, when no such importation can be established? Thus is this vaunted "affirmative testimony" overturned by the positive contradictions of Drs. Ferguson and Bancroft, and falls into ruins of itself from the sandy nature of its foundation.

I know not whether the numerous egregious errors, and dashing groundless assertions of this writer, are attributable to a flippant ignorance, or perverse malversation. Painful as it is to see any individual with pretensions to respectability, and powers to make himself esteemed and respected, place himself in the predicament of being hung on either of the horns of so worrying a dilemma, it becomes unavoidable, from the nature of the attack made on me, and the defence which I must employ.

The reference made by Dr. Hosack to Diemerbroeck, Rondeletius, Clavigero, and Howard, in order to prove, that animal matter in a state of putrefaction "will not generate *pestilential fever*," I instanced as strong proofs of the inaccurate reference to authors, for opinions and facts not contained in their writings.

I was fully persuaded, that Dr. Hosack had not read either Diemerbrock or Rondeletius, when he cited them as testimony, with so much confidence, and, that he had been led into the error he committed, by a careless perusal of Dr. Ferriar's paper, on the "Origin of Contagious and New Diseases." For the information of Dr. Hosack, with a view to guard him against a repetition of the blunder, I stated concisely the particular opinions of Diemerbrock, which I sustained by quotations from his work. The Belgian philosopher distinguishes between plague "pestis," and pestilential and malignant fevers—"febres pestilentes"—"febres malignæ." The first, "pestis," does not result from natural causes, but proceeds immediately from the most just anger of God, "justissima summi Dei ira," for the sins of men. Dr. Ferriar's essay could have furnished this information, had it been read with attention. "Diemerbrock, Dr. Willis, and some other eminent medical writers of the last century, supposed the *plague* to be *always an affliction from the Deity*."* It is to support this opinion, that Diemerbrock mentions the fact, that animal matter in putrefaction, will not generate *plague*, although pestilential fever is not an unusual consequence. It is this last circumstance, that gives the point to the argument of Diemerbrock, which would be futile in the extreme, with the interpretation of Dr. Hosack, and his obstreperous advocate. The expressions of Diemerbrock, are "*nulla tamen pestis insecuta est*:" "*nulla peste subsequente*"—no *plague* succeeded—no *plague* followed. The word "pestis," is used by Diemerbrock to designate the plague. In no instance does he employ it as meaning pestilential fever.—"Febris pestilens," is the phrase with which he uniformly expresses himself, when speaking of pestilential fever—a distinct disease, as he considers, from the plague.

* Medical History and Reflections, first American edition, page 119.

The very next sentence to the quotations of Dr. Ferriar, copied by the writer of the Remarks, is conclusive, as to the truth and correctness of this representation of Diemerbrock's views; while the whole of the concluding part of the paragraph, is hostile to the theory of Dr. Ferriar, which shows, that, even that respectable writer had not, with perfect fairness and candour, quoted his authority. "Si aliquando," is the language of the sentence succeeding the quotation of Dr. Ferriar: "*ex scdâ cadaverum putredine aliqui morbi oriantur, illi vel erunt putridi simpliciter vel putridi maligni ac pestilentes privati, non autem vera pestis. Liqueat id ex historiâ Paræi citatâ, qui ex cadaveribus in puteum projectis inquit multos tanquam peste interiisse; non dixit simpliciter peste, sed tanquam peste, id est, morbo tali quem propter malignitatem assimilât pesti.*"* No pertinacious cavilling, can invalidate the force of these expressions, and they must put to shame the bold animadverter, who with the most perfect assurance pretends to speak of the opinions and doctrines of a writer, whose works he has never seen.

It is easily to be perceived how Dr. Hosack has been led into his error by mistaking Dr. Ferriar's theory, for Diemerbrock's opinion, in consequence of his ignorance of the works of this last author. Dr. Ferriar observes, "it is unnecessary to mention some of the principal theories relating to the rise of plague and pestilential fever. *I place these together, for I apprehend the plague to be a fever, attended with some unusual symptoms, chiefly produced by its violence.*"† This opinion Dr. F. formed, he says, principally from Diemerbrock's cases; but it is his own view, and not that of the Belgian physician. Had Dr. Hosack cited Dr. Ferriar as the authority, he would have been perfectly cor-

* De Peste Caput, viii. Problema iv—4.

† Medical Histories and Reflections, American edition, page 116.

rect, but to display his erudition, he quoted Diemerbrock, who is directly opposed to him.

That Diemerbrock was decidedly of opinion, that animal matter in a state of putrefaction did generate pestilential fevers, the several passages I referred to in his work, "*De Peste*," must have satisfied the most prejudiced. The facts he mentions are positive; his language unequivocal—"putridos fœdores *semper febres pestilentes* inducere et experientia docet." Can words be more explicit? The references and quotations I selected are unimpeached by the reviewer of my essay, yet, thus admitting their correctness, with this perspicuous language in which Diemerbrock delivers his opinions placed before him, he has the audacity to deny that such are Diemerbrock's doctrines.

The gross errors and misstatements of the defender of Dr. Hosack, as it respects the sentiments and doctrine of Diemerbrock, I should be charitably disposed to believe, were the result of his acknowledged ignorance of the work, of whose contents he speaks so confidently; and his miscomprehension of Dr. Ferriar. But what apology can we frame to excuse, or what plea imagine to explain his tergiversation as regards Rondeletius; and the object of Dr. Ferriar's citation of this author: both of which are mistated, with an obstinate perversity. Dr. Hosack is the only person, who ever thought of mentioning Rondeletius, as an authority to prove, than animal matter in a state of putrefaction, would not generate pestilential fevers. Rondeletius, in no part of his works, expresses an opinion on the subject, and the only fact he mentions, that has a relation to it, proves the reverse. The quotation was given in my essay, and need not be repeated, especially as it is not challenged by my reviewer.

Dr. Ferriar, when discussing a question entirely different, viz, the power of a dead body to communicate *infection*, cites Rondeletius to prove, that it cannot. The language of Dr. F. is too clear to admit of any misapprehension. "Another

question, connected with this (whether contagion assimilates all the fluids to its own nature) and illustrative of it, is, whether the dead body of a person destroyed by a plague or fever, be capable of communicating infection. On this subject facts are wanting. Rondeletius (as quoted by Sennertus) asserted that he had dissected bodies dead of the plague, in the presence of many of his pupils with perfect safety. * But let Rondeletius speak for himself. "*Quare corpora illa mortuorum, quæ amplius non expirant nullum venenum ejaculantur. Quod si aliquid contrahatur, hoc potius ab eorum pannis contrahitur; quam ullâ re aliâ. Siquidem dissecuimus aliquando corpora mortuorum ex peste, multis spectantibus studiosis sine aliquo damno; propterea quod mortuo animali perit omne venenum.*" From some extraordinary inadvertence, Dr. Hosack evidently misunderstood the passage that has just been extracted from Dr. Ferriar's essay, and quoted Rondeletius in his discourse on Medical Police, as an authority in support of his position "that animal matter will not generate pestilential fevers." The error was pointed out in my essay, the entire irrelative-ness of the observation of Dr. Ferriar, as well as of Rondeletius, to the subject that Dr. Hosack was discussing, was shown. A single glance at Ferriar, must make the correctness of my remarks manifest to the dullest capacity. What is the course pursued by our veracious reviewer? He insists that the fact mentioned by Rondeletius is demonstrative that animal matter will not generate pestilential fevers, because dissecting a body recently dead of the plague the contagion of plague was not communicated by it. But he goes further. He avers, that Ferriar viewed the testimony of Rondeletius in the light that Dr. Hosack had placed it, and quoted it to show, that animal matter in a state of putrefac-

* Medical Histories and Reflections, American Edition. Page 124.

tion will not generate pestilential fevers." Now mark the deliberate deception practised. The only passage of Dr. Ferriar's essay, in which Rondeletius is mentioned, is the one that has been given above. But instead of quoting this passage, to sustain his solemn asseveration, he extracts a passage from another part of Dr. Ferriar's essay, in which Rondeletius is neither mentioned nor alluded to, and where a subject is treated wholly different from that, to support which he appeals to the evidence of that writer.

A deception precisely of the same character, as that which has been exposed in the instance of Rondeletius, is practised with respect to Howard's testimony. An observation of the distinguished philanthropist to show that the contagion of plague," is not communicated when the corpse is cold, of a person dead of plague," is perverted, by Dr. Hosack's partisan, to mean that animal matter will not generate pestilential fevers." The note which the reviewer transcribes from the work on Lazarettos, will bear no other interpretation than that I have given to it, but the text to which it is connected rendered the intention of the author more explicit. "It is by these ideas of the communication of the plague that the foregoing rules have been suggested." *

It is lamentable to behold such a *felo-de-se* of character on so trifling an occasion. This writer seems as though scorning every species of management, by which his reputation might be covered, he would throw off all reserve, and bare his forehead to the brand of shame.

One more authority remains to be examined, and this disgusting exposition of quibbling sophistry, and misrepresentation is terminated. Clavigero as well as Diemerbrock and Rondeletius with the appearance of great erudition and laboured research, was quoted by Dr. Hosack, as an author, whose work he had consulted. It now appears that this writer,

* Dr. Howard on Lazarettos. Page 25.

and I presume Herrera also, was known to him, as well as the other authorities, only at second hand. On examining this writer to test the correctness of the reference made to his publication, I found nothing to justify it, but facts chiefly opposed to the doctrine of Dr. Hosack. How is this answered? by appealing to the truth of the extracts, I made from Clavigero, or an attempt to point out their incongruence to the subject for which I produced them? Not a word is said in opposition to the accuracy of my quotation, or to lessen the overwhelming force with which they prostrate the reference of Dr. Hosack to the history of Mexico." To meet the positive testimony I adduced from the work of Clavigero, a negative fact is produced which had been employed by Dr. Chisholm for some object, in some of his works into which I feel no necessity of inquiring, as negative evidence cannot be admitted to possess the slightest weight opposed to testimony of the most positive character.

I have now completed this part of the task that has been imposed on me, by the necessity of self vindication. It has been by no means a grateful employment. I possess not that species of taste, which derives a gratification in displaying and contemplating the "human form divine" blurred with deform-
edness. Infinitely more ungracious has it proved to my feelings, though in the strict performance of duty; to make bare this picture of moral disfiguration. The charitably disposed reviewer, in borrowing the language of Junius fancies, that he becomes clothed with the powers of vindictive vengeance with which that eloquent but uncandid and often malignant writer was so liberally endowed, and which he so often unsparingly exercised; but the borrowed tinsel can impart no sharpness to the leaden dart. The bed of torture "he prepared with so much complacency, I am afraid, he is doomed himself to occupy. If not callous to every emotion of shame, the public exposure, he has called down on his head must awaken a keen sense of shame and

give birth to the deepest regrets. Let him employ the severe lesson he has received to its proper purpose, and learn, that no circumstances can justify or palliate, especially in a Medical writer, the slightest deviation from the most religious adherence to candor and truth.

I am perfectly sensible that the subject of this discussion is of little moment. The knowledge that consists in an acquaintance with the opinions of writers, is of the lowest species:* although it is that, which, carrying with it all the weight of erudition, most frequently imposes on the judgment of mankind. Dr. Hosack attempted in his "Discourse on Medical Police," a liberal display of this kind of learning. It made no inconsiderable impression on the public, spread before it through every medium, and on a subject, the correct decision of which, involves its highest interests. A slight acquaintance with some of the writers, to whom Dr. Hosack so confidently referred, rendered me conscious of very extraordinary and important inaccuracies that he had committed. Engaged in the same discussion, I thought it my duty to correct them. This measure has occasioned me to be assailed with a most intemperate spirit. All my statements are contradicted; I am charged with having never seen the works, from which I made extracts, which of course must have been fabricated by me; with having misrepresented the sentiments of writers, and descended to the mean arts of sophistry and evasion. To repel these injurious aspersions, that, "lost in the labyrinth of their fury," my antagonists have sought to cast on me; this idle and tedious discussion, for such I fear it will be thought by those not immediately interested in it, was rendered necessary. If it be of any consequence to have determined who are the guilty, and by whom deception has been practised, the facts and authorities are now so am-

*This sentence must be understood with some limitation, and is only true when it is used for the purpose of erudite parade.

ply spread out, as to enable every one to form his own conclusions, independent of any cavils and angry comments.

Having disposed of the "Remarks on Certain Parts of 'An Account of the Yellow or Malignant Fever, &c. &c.'" there remain to be examined, tested, and justly valued, the pretensions, arguments, knowledge, accuracy, and truth, of Dr. Ducachet's "Reply to the remarks of J. &c."

The passages of the remarks of J, that touched acutely this gentleman's sensitive faculties, stirred into a ferment the bile of his *meek* disposition, so that, without offending him personally, he has made me the object of his petulant invectives and injurious detractions; why? I am at a loss to divine. But, feeling, and acknowledging the obligation he has laid on me, I would not, that he should deem me forgetful of it, by not returning, to the extent of my poor ability, the debt most deservedly his due.

Was it necessary, however, that I should track, step by step, as in the preceding examination, the doubles of his course, I might be deterred from the labour, disgusted already with its loathsomeness. Happily the unpleasant task is spared me. Without threading the wiles and shifts of an experienced cunning, I shall break his cover, and placing in full view, the varied artifices, skilfully contrived to deceive, strip him of their resource.

Dr. Ducachet's essay purports to be "A Reply to Remarks of J, on the Review of the papers relating to the Fever in New-York, in 1820." To ascertain the relevancy of his statements and arguments, it is proper to determine what are the remarks, which Dr. Ducachet pretends to answer and refute.

The committee of the New-York Medical Society, in a "Report explanatory of the causes and character of the Epidemic Fever, which prevailed in Bancker-street, &c." offered three essential characters, as designating Typhus Fever. The 1st was, that medical writers apprise us that typhus dis-

appears in warm weather ; and that it is most prevalent during cold weather, more particularly if accompanied by humidity of the atmosphere : 2d. that the young and robust are the least liable to its attacks, but the old and debilitated are its most general subjects : and, 3d. that it is unexceptionably a fever of continued type, and of protracted and uncertain stages.

This Report was reviewed in the Medical Recorder for April, 1821, by B, who particularly selected these diagnostics as the subject of his animadversion. The first position he contested, and declared himself entirely ignorant of the writers, who, according to the committee, inform us, that "typhus disappears in warm weather, and is most prevalent during cold weather, more particularly if accompanied by humidity of the atmosphere." Proclaiming his ignorance on this subject, he solicits to be enlightened. The 2d. position of the committee, B admits to be correct ; and the 3d. he meets with cavils and exceptions, instead of argumentation or general observations.

The avowal made by the reviewer of his total unacquaintance with any medical writers, who entertain the opinion attributed to them by the committee of the New-York Medical Society, appeared of the most extraordinary character. This ignorance, whether real, from a limited acquaintance with medical writers, or feigned, for the purpose of sustaining an argument ; equally disqualified him for the office he had assumed. If he truly had not, in the course of his medical readings, met with the writings alluded to by the committee, instead of presuming to criticise the performance of others, he should have been engaged in his studies, and been laying the foundation of his medical information. If his disavowal of a knowledge of these writers was false, then is the character of all his observations vitiated, prostrated in the mire, and stripped of every title to credence or respect. Crediting his declaration of an absolute ignorance in this respect, and the

sincerity of the desire expressed, to be informed, "who the 'medical writers' are, that gave the committee this wonderful information," that information was communicated to him by J. It appears to have been received by B. as it was intended, in a friendly way, for he has not thought it necessary to make any observations in reply. He has no doubt profited by the instruction. Why Dr. Ducachet has thought it necessary to feel himself aggrieved, is quite incomprehensible. Dr. D. asserts, that I "*accused* the reviewer of being ignorant of one of the commonest and best established facts in medicine." This is not the only instance, in which Dr. D. violates the truth. It was the reviewer who proclaimed his own ignorance, and I did no more, than believe what B. said of himself. It is not J, but Dr. Ducachet, that insults the reviewer, by refusing credit to his words; and he does this under the mask of friendship.

That the authorities cited by J, established in the most incontrovertible manner, the point for which he adduced them, viz. to support the first position of the committee of the New-York Medical Society, that there are "*medical writers* that apprise us that typhus disappears in warm weather," &c. is beyond the reach of cavilling captiousness, or crooked prejudice. Dr. Ducachet, by the disingenuousness of his conduct, shows it in the strongest light: he undertakes to controvert the correctness of J's references, but, instead of honourably and manfully meeting the question; by an artifice that would be esteemed disgraceful in the meanest disputant of a tavern forum; he distorts and mistates the point in controversy, in order that he may be able to frame an argument, and find a plausible excuse to give vent to malignant passion, and indulge the disposition to detraction, that forms so conspicuous a portion of his elaborate essay.

A reference to the Remarks of J., will convince any candid mind, that the only intention of the writer, in quoting his authorities, was to demonstrate to the reviewer B. that there

were "medical writers, who apprize us that typhus disappears in warm weather, &c." The establishment of this fact, and the confession of an ignorance of such writers by B, was conclusive evidence of his incapacity for the task he had undertaken. Dr. Ducachet finding the subject placed by J. in a position that was impregnable, resorts to the pitiful evasion I have mentioned, on which he no doubt prides himself, as an admirable ruse de guerre; and makes a new question, viz. "that Dr. Jackson is bound to show that typhus never prevails in warm weather," examines the authorities as to their bearing, not on the point for which they are cited by J. but on his interpolated question; and then accuses me of deceit, because *all* of the authorities *do* not prove to the fullest extent, "that typhus *never* appears in hot weather." Practising this duplicity, Dr. Ducachet dares to talk of honesty!!

In my account of the malignant or yellow fever, in Philadelphia, in the year 1820, I stated as a diagnostic of typhus, "that it never does appear *epidemicallly* in hot weather, and ceases on the approach of cold." I have no hesitation in admitting, that the position as laid down, is too general; that it does not apply uniformly to the variable climate of England, or some other temperate climates equally marked by versatility. But, it is apparent from the context, that J. was alluding to typhus in this country, and not in cold, moist climates. As it respects this country, where the thermometer averages from 78 to 83° during the summer, I hold the observation to be incontrovertible, as well with respect to other climates of an equal or greater temperature. Had it been my object to have supported the position, "that typhus never appears epidemicallly in the hot weather of this country, or of warmer climates, as Dr. Ducachet would have it that I was bound to do, (why? it is impossible to conceive), different authorities and arguments would have been resorted to.

In that case, I should have undertaken to have shown, that genuine contagious typhus, the disease to which the name is given by Cullen, and to which it should be strictly confined, is absolutely unknown in the hot summers of this country. I should have shown, that under the denomination of typhus, in this country, and it may be added in the West Indies; are vaguely included several species of fevers, such as the gastric gastro-enteritic; the mucous fever, febris pituitosa of Stoll, adéno-méningée of Pinel; the slow nervous fever and others, are all, without the least discrimination, classed as typhus, especially complicated, as they so frequently are, with an adynamic state of the system. In these instances, the type of the disease or state of the system, is confounded with the diagnostic symptoms—the accident is taken for the essence. I would have established by indubitable authorities, that in Africa, in India, throughout a large extent of Asia, countries including a population of from 2 to 300,000,000, under the most favourable circumstances of filth and confined situations, crowded with human beings, *that* fever is unknown; and when carried thither, cannot be propagated. While on this subject, without entering at large into its discussion, a single observation of a most competent authority may be cited.

“Before dismissing the subject of fever, I may observe, that no well-marked case of typhus occurred to us in Egypt. In India we never saw a case of this species of fever. To the existence of this fever, which in Europe has committed such havoc in our fleets and armies, the *climate of India is inimical*. We know instances where, in transports, typhus had broke out, and, on the passage to the Cape of Good Hope or India, had proved little less destructive than the plague could have done; but the disease never reached India. If a case was landed there, it never propagated the contagion: a second case never appeared on shore. On inquiry, I found that no case had ever been known on the wes-

tern side of the peninsula, nor have I ever heard of its existence in the eastern." *

Dr. Ducachet was not contented with resorting to the deceptions practice I have exposed, but he also attempts a similar imposition in individual instances. Sir Gilbert Blane prides himself, as having been the first, who noticed the fact, that "there is something in tropical climates unfavourable to the production and continuance of infectious fevers."† Dr. Ducachet has not the hardihood to deny openly, the correctness of the quotations I made from Sir Gilbert Blane's work. The first and third quotations are as positive of his belief, of the effect of a warm climate in destroying typhus, as can be expressed in the English language. Dr. Ducachet does not pretend to controvert this accuracy; he passes them by without notice, although embracing the main and important part of the question, and only notices *six words* of the second quotation, and those but correlative, as though they constituted the only quotation, and the only opinion for which Sir Gilbert was referred to. And yet, Dr. Ducachet has the face to pretend to honesty as a controvertist!!

But this is not the only deception practised with regard to this writer. Dr. Ducachet asserts, that "in his observations on the diseases of Seamen, he (Sir Gilbert) tells us of the existence of a low ship fever on board of two vessels on the West India station, in the June of 1780." By referring to the "Observations on the Diseases of Seamen," it will be found, that Dr. Ducachet has grossly misrepresented the observation of Sir Gilbert. The words are, "The fever in these two ships *resembled rather* the low ship fever of Europe, than the bilious one peculiar to the climate."‡ And Dr. Ducachet is an honest man!

* McGregors Medical Sketches of the expedition to Egypt from India, page 169.

† Observations on diseases of Seamen, edit. 1785. p. 287.

‡ Observations to edition 1785, page 30.

Other misstatements, were it necessary, would be exposed, but they would swell this communication, already too extended to an unwarrantable size.

“*Crimine ab uno, disce omnes.*”

The disingenuous proceeding of Dr. Ducachet, in misstating the arguments of J. and the sophistry of which he is guilty by replying to those misstatements, as though they had proceeded from me, and testing the accuracy of the authorities quoted, by the same standard ; have been amply exposed.

It is deemed quite unnecessary to occupy any additional space, in illustrating the adaptation of the references to the points, intended to be established by them. It is conceived, that they are not invalidated in a single circumstance, by the cavil and objections brought against them by Dr. Ducachet, which are evidently the result of a want of extended and general information, as to the diagnostic symptoms of the different febrile affections.

The utter impossibility of ever arriving at truth in any discussion, without employing terms in a certain and defined sense, is too obvious a principle to require an argument for its support. To avoid the errors and misconceptions arising from the use of terms in a loose, general, and different meaning, two positions were laid down by J. The first was, that there is a disease answering completely to the definition that Cullen gives of Typhus, of which contagion is an essential attribute. The term typhus having been bestowed on this disease, and universally adopted, should be confined to it exclusively. The second was, that diseases will often assume various types without losing their specific characters. Thus gastric, mucous, yellow, slow, nervous, and other fevers, will appear with a typhoid or adynamic, or an ataxique type. It is common to mistake fevers with these complications for typhus, though they are very distinct. The following extract from “*Medical Sketches*” of Mr. James M'Gregor, presents a striking example of the various types that are

sometimes witnessed in a single disease. "In the Indian army, when the disease (plague) first broke out, the cases sent from the crowded hospitals of the 61st and 88th regiments, were from the commencement attended with the typhoid or low symptoms.

The cases sent from the Bengal volunteer battalion, and from the other corps, when the army was encamped near the marshy ground at El Hammed, were all of the intermittent or remittent type.

The cases which occurred in the cold rainy months of December and January, had much of the inflammatory diathesis. Mr. A. Whyte remarked, that every case admitted into the hospital at Rahamania, had the symptoms of pneumonia.

In the end of the season, at Cairo, Ghiza, Boular, and on crossing the isthmus of Suez, the disease wore the form of a mild continued fever." *

In all these instances, the plague never lost its distinctive character, though presenting so many aspects or types. Similar observations have been repeatedly made in other diseases. Dysentery will be sometimes attended with a highly inflammatory diathesis, and require actual depletion; at other times, it is accompanied with a typhoid or adynamic state, and demands a totally different management. Even the symptomatic fever, succeeding upon large wounds, especially gun-shot wounds, is frequently of a typhoid character.

The principles that have been stated, were laid down by J., to anticipate and obviate the objections and arguments that are employed by Dr. Ducachet. Those principles he has not pretended to controvert; yet heedless of these distinctions, that are sustained on the highest authorities in medicine, in a most strange confusion, he jumbles together fevers that are totally distinct. If an author employ the term typhus, though the symptoms described demonstrate the disease to have been gastric, slow nervous, mucous, or other fever com-

* Medical Sketches of the Expedition to Egypt from India, page 3—112.

plicated with an adynamic or ataxique state of the system, or, as it is in this country termed, typhoid type, it is seized on with avidity. If the author gives another name than typhus, as Hillary does to the fever of which he treats, Dr. Ducachet insists it is typhus, and typhus it shall be, whether the symptoms correspond to that disease or not. By this most ingenious manœuvre, as the candid Doctor no doubt thinks it is, he creates typhus, whenever and wherever it will suit his argument. But, dexterous as is the stratagem, the liberality with which he has employed it, and the monstrous incongruities of which it is productive, entirely destroys the effect it was intended to accomplish. No one is deceived, unless it be the Doctor himself; while his reputation as a medical observer, as a physician versed in the elements of his profession, and capable of distinguishing the discriminative features of different febrile diseases, without which, no one merits the character of a physician; is implicated by his own art—is sacrificed to the petty artifices of a controvertist.

The pyretology of Dr. Ducachet, appears like that of De Haen, to consist of but two fevers. A fever without acute inflammation, is, with him, typhus. Thus, the slow nervous fever described by Hillary, ("known to be" such, and not typhus, "from the description he gives of it," by all who are acquainted with the characters of the two diseases); the fever mentioned by Lind, as "*approaching nearest* to what is called a nervous fever;" the slow nervous fever of Huxham, *febris mucosa—adeno-méningée* of Pinel; the gastric fever of our summers and autumn, complicated with adynamic symptoms, so frequently described by Dr. Gallup, it is true, under the name of typhus; the same disease in the Philadelphia almshouse in 1820, described by Dr. Joseph Klapp—all these, and other fevers of different characters; of species wholly distinct; by the sinister management of Dr. Ducachet, are in utter confusion, heaped together, and confounded with ty-

phus. He breaks down, through an irreverent vanity, or unskilful ignorance, the ancient land marks of the science, consecrated from the earliest period, by all that is revered and esteemed in medicine, for genius, experience, observation, and truth. The beautiful harmony, and exact disposition, that now manifest themselves in the classification of diseases, through the prevailing influence of an analytical observation in medicine, would be dispersed, and "chaos come again," could the legerdemain tricks of the "Reply" of Dr. Ducachet possess an influence, equal in their magic to their pretensions. Devoid all discrimination, the various species of fever have lost, in their levelling system, their peculiar characteristics—"nulli sua forma manebat."

With a view to prop the most pitiful of all cavils, resting on a want of discrimination of the terms typhus and typhoid, Dr. Ducachet most pertinaciously insists, "they are absolutely synonymous." Nay he is bold to affirm, that "no author of established repute in medicine, recognises the difference contended for by Dr. Jackson." Notwithstanding this bold affirmation, a few lines subsequent, he "grants, indeed, that," typhoid, "when used by medical writers, is seldom intended to designate typhus fever itself, but rather some disease in which the fever is of a typhus character"!!! Can any thing be so ridiculous as the bold affirmation, and this palpable contradiction of it, jumbled together in the space of a few lines. Dr. Ducachet, amongst the rest of his honours, appears ambitious of adding the cap and bells. The prize will be accorded to him without contest.

After so many centuries of fruitless observation, on the ever fluctuating ocean of undefined terms, of idle opinions, of unmeaning words, and conjectural opinions; the introduction of sound philosophy into medicine, has placed the science on the immoveable basis of observation and experience.

At this day, when clearness, precision, and order, are esteemed essential requisites of the science, the inexact and

inaccurate views of fevers, displayed throughout the laboured essay of Dr. Ducachet; his total want of discrimination between things of a distinct nature; his practical efforts to prolong the disgraceful era of confusion and discordancy, too long lamented; and his abortive attempt to justify a loose and negligent employment of terms, cannot be tolerated. He places himself under the censure of every medical philosopher, and friend to the advancement of truth. The practice is justly condemned, and its object properly designated, by the enlightened and profound author of the "*Nosographie Philosophique*," with whose expressive and appropriate language these observations will be closed. "C'est une heureuse ressource pour un esprit peu exact et peu propre à mettre de la justesse dans les expressions, que l'usage de certains termes d'une signification indéterminée, et qu'on peut employer à tout propos sans crainte d'être trouvé en défaut."*

Note.—I should not have felt justified in introducing the name of Dr. Ducachet into the discussion, but for the circumstances already related. I conceive, that, by his conduct, he has outlawed himself, and is not entitled to the observance of those rules of literary courtesy, I ever wish to obey. A question of science can be discussed, without any mixture of personal reflections. When the discussion is anonymous, the obligation to abstain from them is more binding. Without the slightest offence having been offered to Dr. Ducachet, he assailed me with a degree of rudeness, violence, and even maliciousness, for which it would not be possible to account, had not others, respectable for their age and standing in the profession, been equally the object of his coarseness. The law of retaliation, I hold to be as necessary to preserve decorum in the republic of letters, as of manners in society. It is a duty to visit this law on the disturbers of either community; and, if there is exhibited a harshness in some of my remarks, that, in other circumstances, would be unbecoming, my apology must be, that it was the discharge of a painful duty, in return for an unprovoked and wanton outrage.

* Vol. 1st. page 186.

An Introduction to the Practice of Midwifery by THOMAS DENMAN, M. D. *Licentiate in Midwifery of the College of Physicians, London, and honorary member of the Royal Medical Society at Edinburgh, from the last London edition, revised by the author; with notes and emendations by JOHN W. FRANCIS, M. D. Professor of Obstetrics and the diseases of Women and Children in the University of New-York, Member of the Medical and Chirurgical Society of London, &c. &c.* 8vo.—New-York, E. Bliss & E. White—pp. 684.

If there is any one branch of medical science, which can take precedence of the rest in the amount of its improvements, and consequent amount of benefit conferred upon suffering humanity within the last fifty years; if there is any one, which, without danger of extravagant pretensions, can claim to have been productive of more good than ill, from the time of its first cultivation as a science; it is surely that, which has for its object the successful treatment of pregnant and puerperal women. In the primitive stages of society, it might have been a question, whether the interference of art, with the view of shortening the duration, or alleviating the pains of gestation or parturition, was ever necessary, seeing that the female sex in either of these situations were barely fulfilling a law of their nature in no way necessarily connected either with vice or disease; and if we except the effects of accidents and the remedial measures which they suggested, the question might have been answered in the negative; but this state must have been in all countries of very limited continuance, since savagism itself has its vices and its hardships, its privations and its luxuries, all of which would soon become fruitful sources of disease, deformity, and consequent difficulty; and call for the exercise of some expedients for the relief of the complaints inseparable from the condition of female existence. These expedients collected, remembered, and orally transmitted from the mother to the

daughter, from generation to generation, would finally form a body of traditionary facts, which would at no great distance of time, become so numerous, varied, and perhaps so contradictory, that their knowledge and application could not conveniently consist with the ordinary avocations of any individual female. A necessity would thus be created of committing the management of women to particular persons, who would, in process of time, be the sole depositories of this species of practical wisdom. Accordingly, we find that midwifery as a profession was very early practised, and with a success, as far as we are able to judge, surpassing any other branch of the healing art. It may appear a strange position, and yet strange as it is, we doubt whether it can be successfully controverted; that the success attending the practice of midwifery in former times, is chiefly to be ascribed to the fact, that the exercise of the art was confined to the female sex: whence the treatment may be supposed to have been merely of a negative character, depending more upon preventive than curative management, and consisted not in always doing what was right, but in abstaining from doing what was wrong. If peradventure, the officiousness of the midwife was productive of inconvenience and injury, as must occasionally have occurred, the recollection of the evil, and the consequences which it entailed, would quickly operate to secure the patient against a repetition of a dangerous practice; so that in place of its becoming a precedent, for the guidance of others, as has too often happened in these days, every untoward accident, or palpable case of mismanagement, would serve a valuable purpose in deterring others from committing similar follies; for it is fair to suppose that success in practice was the only criterion of individual merit. To this it may be objected, that there are very many cases in which women from their natural timidity and want of knowledge of the relative situations of the organs concerned, were absolutely incapacitated to render any effective as-

sistance ; and that therefore, all such cases of difficulty as rendered instrumental delivery necessary, must have terminated unfortunately, and perhaps fatally, both to the mother and to the child : but even allowing the objection to have its full weight, and to be received without any qualification, it could not invalidate the general position, unless it could be proved, that instrumental delivery was only resorted to, when the necessity for its exercise was indispensable.

From a comparison of the various registers of lying-in wards, and out-door charities, it will be found that of every 100 births, 90 of them would progress to a happy termination as respects delivery merely, if left altogether to the efforts of nature, and no artificial assistance of any kind were afforded, and of the ten which might require it, 7 would terminate favourably by the operation of turning or some slighter manual assistance. From a report of the practice of midwifery at the Westminster general infirmary, during the year 1818, published by Dr. A. B. Granville, it appears that even this estimate is by far too large ; for of 640 females, 619 were safely delivered without the slightest interference, by nature alone : giving a proportion of little more than 3 1-2 in 100. So that if we allow midwives to be so far educated, as to perform this essential part of an accoucheur's duty, we have remaining no more than three per cent at most, of all parturient females, who would die undelivered, or become the victims of ignorance or temerity.

If a calculation of this kind can be received as nearly approaching the truth ; it may be asked, where is the man who is prepared to say, that less than this proportion will suffice to cover the fatality, which has resulted from that unwarrantable, and we had almost said wanton abuse of instruments, which disgraced the practice of midwifery, but little more than half a century ago ; for it is to be remembered, that the effects of instrumental delivery, more especially when performed awkwardly or inconsiderately, are not con-

fined to the single occasion in which it is practised ; if it were so, it is highly probable, that notwithstanding its abuse, its benefits would, upon the whole, have more than counter-vailed its evils ; but they may be, and are justly chargeable as causes of future difficulties, which, but for them, might never have been experienced.

Whatever difference of opinion may exist, on the subject of the comparative success of midwifery, in the hands of the male or female practitioner, previous to the time that it was first publicly taught in London, which is not more than one hundred years ago ; it is certain, that very soon after this period, and until the time of Dr. William Hunter, the evidence is by no means equivocal, that humanity had gained very little by the transfer ; for we are told that some physician accoucheurs used instruments, as frequently as three times a week, and that the green bag, so admirably caricatured by Sterne in his memoirs of Tristram Shandy, was the indispensable appendage of every man-midwife. It is as unnecessary as it would be unpleasant, to dwell upon the evils which attended the introduction of instruments, in the practice of midwifery, and the more so, since it must be obvious that few of them could be attributed to, and many of them might have been avoided by, a just and proper estimate of the circumstances which required their application.

The few preceding remarks have naturally suggested themselves, on reading the book whose title stands at the head of this article, and appear to us a very proper introduction to the notice of a work, which has repeatedly received the seal of public approbation. It cannot be expected of us, that we should attempt an analysis of that, which is already familiar in detail to every physician, who claims to have acquired a competent knowledge of the different branches of his profession, and whilst on the one hand, we would not wantonly insult the understanding of the respectable practitioner, by supposing him

ignorant of the merits of this book ; we on the other, cannot be so indifferent to the interests of the medical student as to wish to do that, for the purpose of economising his time, which must manifestly result in as much injustice to the author, as injury to himself. Dr. Denman's midwifery is a work of that sterling stamp, as to render a synopsis of it within any reasonable compass absolutely impossible ; his reasonings are so diffuse, and yet so dense ; and his details so minute, and yet so so essential, as to render it a matter of no small difficulty to abstract without mutilating them—the faults of the book, and what book is faultless ? are those of omission, and his is the rare merit, (except in two or three instances,) of never misleading his reader on a point of practice ; though we may occasionally regret his deficiencies, on some very important subjects of an accoucheurs' anxiety, which we should have been pleased to have seen more largely treated in the notes of Dr. Francis : such, for instance, as the collapse of the system after delivery, and the treatment of hemorrhage, after the removal of the placenta ; his doctrine of the evolution of the fœtus in utero, &c. The profession are greatly indebted to Dr. Denman, for the bold and decided opposition which he manifests against that too frequent, and therefore, in many instances, injurious application of instruments to facilitate delivery, which was the necessary consequence, of the almost unbounded popularity which the works of Chapman and Smellie had severally acquired, and which, but for the overwhelming weight of a Hunter's character, might have continued to degrade the practice of the art to this day.

The work itself, independent of its intrinsic excellence, derives an additional interest from the consideration that the professor of midwifery in this college, has added to the last London edition, many notes, both critical and supplementary, and purposes to make it the text book of his course ; and a better, in our opinion, he could not have chosen, as it com-

bines, with a neat and systematic arrangement, all that lucidness of detail calculated to recommend itself to the student. And it is moreover better suited to the condition of society in this country, than most of the publications on the same subject, which have issued from the British presses; and, although Dr. D. might not have been aware of this circumstance when he wrote it, it is nevertheless true, that the application of instruments in the practice of the art, other things being equal, is much less required here, than in almost any part of Europe; for we have few deformed children born among us, and still fewer deformed parents: so that if the most judicious use of instruments was entirely abandoned, the practice of midwifery would show a much more favourable result than that of any section of modern Europe. That this is true, is beyond a doubt, and the causes are as obvious as the fact is indisputable. The inhabitants of this country were originally, and they are not so far degenerated, that they may not be considered still, collectively a healthy, athletic, well formed, and robust people, composed of the immediate descendants of those who were induced, either by necessity or commercial enterprize, to abandon the land of their fathers, in search of comforts or conveniences which were denied to them at home; and among this number are rarely found the diseased, deformed, or valetudinary. A large proportion are emigrants, accustomed to hardships and fatigue, whose exertions in the pursuit of happiness and wealth, were necessarily cramped by the disabilities naturally incident to their condition at home, or unavoidably imposed by the political state of the countries which they had left. Perhaps there is not a single government of Europe, whose population has not been taxed to furnish inhabitants for this; and of that kind too, as when considered merely in their physical character, cannot but be estimated as of the most effective classes. And when to this we add the prevalence of early marriages, before the female system has taken on that

rigidity of fibre, which marks the middle periods of life ; the near prospect of early settlements ; the scarcely less distant one of the possession of comparative comfort and conveniences attendant on them ; and the almost incalculable moral influence of contentment, we have cause sufficient to account for the rare occurrence of cases of difficult parturition, as arising either from disease or deformity : without recurring to that doctrine which Buffon inculcated, that the crosses of the blood were among the most efficient causes of the improvement of the stock. The truth of this last position is evidenced, by the repeated experiments which have been made in the rearing of domesticated animals ; and if it be allowed to have any foundation in fact, in respect to our own species, surely the population of this country, from its very constitution, may be supposed to participate largely in the benefits of this bountiful provision of nature.

Dr. Denman's book is one of those rare productions, to which genius and experience have equally contributed ; and it offers the most unequivocal evidence, that both have been submitted to the dominion of that conscientious regard to duty, which seeks its reward from a higher source, than the commendation of his profession, or the world, whilst it solaces itself in the satisfaction arising from the purest intention, to benefit suffering humanity ; and the modesty of the author upon points merely speculative, or as yet unsettled, is only equalled by the confidence which he evinces, on such points as his observation has taught him. On the subject of menstruation, for instance, which has occupied a large share of the attention of almost every writer on midwifery, Dr. D. scarcely spends one page ; while on the subject of natural labour, his essay is elaborate, his reasonings conclusive, and his opinions assured and settled : and we hazard nothing by asserting, that the student, may in vain look elsewhere for a more finished exposition of this important process. The introductory remarks on difficult labour, is a beautiful spe-

cimen of fine writing, as well as correct reasoning, calculated to rivet the attention of any reader who feels any concern in the subject of which he treats ; and from among many observations equally true, though not so humiliating to the profession, we will quote one, which ought to make a lasting impression on all obstetric surgeons, who incline to the belief, that eminence in the art depends upon the skill evinced in the use of instruments. " It must, however, be acknowledged, that all the errors of practice do not proceed from ignorance of the art. Some of them may justly be imputed to our entertaining too high an opinion of the art, or its application in practice, when it is not absolutely required, and when its application is prejudicial ; to too much confidence in our own dexterity, and too little dependance on the natural efforts and resources of the constitution." That this is true, no man will doubt, who will be at the trouble to acquaint himself with the history of the science, as formerly practised, or even to refer to some recent publications having for their apparent object, its improvement. But as we have no intention to analyse the work, we forbear to remark further on the merits of the text ; our respect for the author is of the most exalted kind, and our estimate of the performance such, as no more fashionable publication is calculated either to qualify or diminish ; and we most heartily recommend it to every man who aims to practise this branch of his profession, with credit to himself, and with a conscientious regard to the duties he owes to the suffering sex, who are the immediate subjects of his attention.

The notes which Professor Francis has interspersed, are, with a few exceptions, very judicious, and the result of much reading and reflection, and offer no equivocal evidence of his intimate acquaintance with the art which he is appointed to teach ; they are for the most part short, and this brevity is their greatest fault : we should have been better pleased if he had extended them more, even if he had been obliged

for this purpose to reduce their number, although we can scarcely blame him for not spending his strength in annotations, if he purposes at some future day to claim the merit of an original writer on this interesting branch of a medical education. It is evident that he does not so much intend to illustrate his author, as to make up his deficiencies; in some points he has differed from him, particularly such as were dubious, from the limited number of facts on which they rested; and we do not hesitate to declare, that in all those instances, he has rendered an important service to the reader. His tabular appendix is rendered peculiarly interesting, as it serves to show that the opinions of his author on the subject of instrumental delivery, are in perfect accordance with the experience of the most celebrated accoucheurs.

The aphorisms are inserted without note or comment.

Before closing these brief remarks, we deem it incumbent on us to acknowledge our obligations to the editor for the condensed view which he has given us, in continuation of the history of midwifery. It certainly was incomplete without his interesting addition, and while it serves the purpose of a scale by which to estimate the progressive improvement of the science, it offers no small testimony to the merits of Dr. Francis in this particular species of writing.

The above remarks on Dr. Francis' Denman would have appeared in the last number of the Medical Repository, but the manuscript was mislaid by the printer, and not found till it was too late to insert it. ED.

COLLECTANEA CLINICA.



A Case of Puncture of the Femoral Artery a little below Poupart's Ligament, for which the External Iliac Artery was tied. Communicated by JOHN NIELSON, Jun., M. D. Resident Physician of the New-York Hospital.

William Smith, aged 49, a private in the U. S. service at Governor's Island, was admitted into the New-York Hospital on the 13th of March, 1822. Thirteen days previous to his admission, in a scuffle with a fellow soldier in the guard-house, whither he had been ordered for misconduct, he received a prick of a bayonet, which wounded the femoral artery of the left thigh, just below Poupart's ligament. A profuse hæmorrhage ensued, and, as was judged, about a quart of blood lost, before it could be suppressed by compresses and the tourniquet. After some hours, the pain of the tourniquet becoming severe, it was removed; no hæmorrhage followed, and the wound seemed to do well. About five days after, however, the hæmorrhage again took place, and was restrained as before. On the twelfth day, the bleeding broke forth a third time, when the tourniquet was again applied, and the next morning he was sent over to the New-York Hospital.

At this time he was quite weak from loss of blood with a considerable degree of febrile excitement from the pain of the tourniquet, which he complained of as being very severe. The thigh, from the knee upwards, was swollen to double its natural size, and a degree of tumefaction extended up the

abdomen. A consultation of the surgeons of the hospital being called, on the day of his admission, it was determined to take up the external iliac artery, the distance between the wound and Poupart's ligament being so small as to preclude the possibility of securing the femoral.

The operation was forthwith performed by Dr. Stevens. The first incision was made midway between the anterior spine of the ilium and the pubis, about two and a half inches in length, inclining with somewhat of a semilunar curve towards Poupart's ligament. The layers of muscles were successively divided, and the sheath of the vessel was exposed without wounding the peritoneum, that membrane being separated from the iliac fascia by the finger. Some difficulty was experienced in the operation, from the ligament being pushed upwards from its natural situation, and the extravasation of blood between the tendinous fascias of the muscles and in the sheath of the artery. A single ligature was passed around the vessel with a blunt needle, directed from within outwards, and the wound dressed with dry lint. Very little blood was lost during the operation, and the patient's pulse, which before had been very rapid, became calmer. He was conveyed to bed; a woollen stocking and cotton wadding applied to the limb to preserve its natural temperature, and a poultice to the wound in the thigh. In the evening, the patient complaining of pain, a sudorific anodyne was administered, and small doses of spirits Mindereri through the night.

March 13. He passed a comfortable night; tongue slightly furred; pulse about 100; some febrile excitement; bowels not having been opened for two days, an infusion of senna with sulph. magnes. was directed, which operated freely; affected limb rather colder than the other—hot bricks were applied. After the operation of the medicine the pulse became less frequent, and the skin cooler. In the evening

the sudorific anodyne was repeated with the spirits Mindereri, and cremor tartar water for drink.

15th. Wound in the thigh discharges freely a bloody matter; limb of natural temperature; swelling much abated.

16th. Continues to do well; incised wound of the operation dressed, which looks very healthy.

17th. Wound in the thigh discharges very freely a healthy pus; to support health under the discharge, directed the bitter infusion; a bandage of strips applied to the thigh to prevent matter from lodging.

19th. Discharge very profuse; pulse rather small with a disposition to diaphoresis; directed decoct. cinchon. with elix. vitriol, and a more nourishing diet; ligature continues firmly attached.

22d. A pulsation this morning was discernable in the anterior tibial artery of the affected leg; wound continues to suppurate kindly.

26th. Continues to do very well; suppuration diminished; wound contracting, and of a very healthful appearance; a solution of sulph. cupri directed to be thrown into a sinus extending between the anterior muscles of the thigh. The granulations of the incised wound being rather pale were also moistened with some solution.

30th. Suppuration very much diminished; general health quite good; solution of sulph. zinc. substituted for the one of sulph. cupri; wound and sinus filling up.

April 2d. The ligature by a moderate exertion of force was detached from the artery, it being the 22d day from the operation.

The wounds continued to heal kindly, and the patient returned to the Hospital at Fort Columbus, on the 26th day after the operation; the wound in the thigh being reduced to the state of a simple ulcer, of an inch in diameter; that over Poupart's ligament nearly healed, and the man capable of walking about, and rapidly convalescing.

A Case of Hydrophobia from the bite of a Rabid Animal. By
S. P. HILDRETH, of Marietta.

ON the sixth of March, 1811, J. Gould, aged eight years, living on the west bank of the Muskingum River, eight miles above Marietta, in the state of Ohio, was bitten on the right hand, in two places, by a dog, which was discovered on the following day to be rabid. The inhabitants were led to this conclusion, from his biting every animal that came in his way, such as cows, hogs, and a colt—a number of these animals subsequently became mad, and were destroyed by their owners. On the second day, I was consulted on the best course to be pursued, for the destruction of the poison, and to prevent its affecting the system. As it was twenty-four hours after the bite was inflicted, I thought it too late to excise the wounded parts, and directed the actual cautery, which was had recourse to, and afterwards an escharotic powder, composed of sulphate of copper, and red precipitate, was applied to the hand, and the part covered with an emollient poultice, till the eschar separated and came out. After this, the wounds were to be dressed with simple cerate, with sufficient of the escharotic powder to keep up a free discharge.

12th. Visited the patient—dressed his hand, and separated the eschars, which came out readily, and left clean, healthy looking ulcers.—Dressed as above directed, and ordered a poultice to increase the discharge.

14th. Saw him again, and directed a small powder of calomel to be taken every third night. He appeared to be in good health, and the ulcers discharged freely.

15th. My patient, after having been playing all day in apparent health, was seized in the evening with a severe rigor, which continued an hour, and was followed with a high fever, soreness in the pharynx, pain in the head, and a difficulty in swallowing liquids.—A slight delirium came on, at-

tended with convulsive motions of the limbs and muscles, which continued all night. He took in the course of the night, twenty-four grains of calomel, in doses of four grains each. It produced one or two discharges from the stomach, mixed with bile, and purged him very severely. The following morning, he could swallow his drinks more easily, but the tremors and spasmodic motions in the muscles, and delirium still continued. The parotid glands were swelled, and his hand much inflamed and very painful.

16th. Directed, ol. ricini, 3j.—and a powder composed of calomel, gr. ss. and pulv. cantharid. gr. ss. to be given every two hours, and continued until a dysury was produced.

17th. Found him much relieved of his most alarming symptoms, and a considerable dysury and tenesmus in their place. Directed the powders to be continued.

8th. The calomel had affected his mouth, and he voided his urine very frequently. The ulcers on his hand looked well, and in order to renew the discharge which had been checked by the inflammation and swelling, they were sprinkled with cantharides. There is an eruption of broad, watery pustules on his left hand, which are hot and painful; also about the lips and nose—His mouth is considerably affected with the mercury—The fever is much abated, and he is fretful and dull, inclined to drowsiness—Directed the powders of calomel and cantharides to be given night and morning, this being deemed sufficient to keep up the excitement in the salivary glands and bladder.

21st. Visited my patient, and found him in a full salivation, spitting so freely as to alarm his attendants with the idea of his becoming mad, from his drooling so much. The ulcers on the hand were healing—applied the escharotic powder, in place of the cantharides. As he complained this morning of a difficulty in swallowing, and that the sight of water was disagreeable, I directed the calomel and cantharides powders to be enlarged to double the quantity. How-

ever, he swallowed well while I was with him.—His bowels are open—has a great aversion to his medicines—the eruption continues on his left hand, but is drying up on his lips and nose.

25th. Found him free from fever—his mouth much ulcerated, and a copious flow of saliva—he is very peevish, and is much harassed by hiccough—Discharge continues from the ulcers.

28th. Discharge from the hand scanty—the escharotic powder to be applied—styalism continues—Rhubarb in small doses, to be taken to keep the bowels open—discontinue the powders.

April 3d. Ptyalism nearly ceased—appetite good—ordered the ulcers to be kept running for a few days—gave no medicine.

In reviewing the above case, I can discover nothing which will induce me to change the opinion I formed of it at the time; that it was a strongly marked case of hydrophobic disease. It cannot be said that the calomel created the sore throat and difficulty of swallowing, as the first dose was given not twenty-four hours prior to the attack—and the difficulty only subsided when the mercury had freely affected the glandular system, and the cantharides had produced a counter-irritation on the kidneys and bladder. To the action of these two potent medicines, may be fairly attributed the preservation of this lad from one of the most horrid and distressing maladies. It is now nearly eleven years since the accident took place, and no symptoms of hydrophobia have returned.

A case of Twins united along the course of the Sternum to the insertion of the Umbilical Cord. By Dr. S. P. HILDRETH, of Marietta.

ON the 15th of December, in the year 1819, I was called to attend on the wife of Thomas Wilson, living in the township of Marietta, and in labour for the second time. She is

a well formed woman, of a full habit, aged about 24 years, and is subject to severe attacks of asthma. The membranes were ruptured, and the waters discharged before my arrival. On examination per vaginam, I found it to be a foot presentation, and that the child was lying with its face forward, or towards the mother's abdomen. As the labour progressed, I made occasional efforts to turn the face to the back, but was unable to succeed. As the child descended within the pelvis, I presently discovered the feet of another child lying with its face to the mother's back. The pains were strong, and I made due exertions to bring forward the first child, before it should become locked in the bones of the pelvis by the descent of the second: but after delivering it to the hips, and using all my endeavour to give it the proper turn, and bring it forward to the world, I was unable to succeed, or to make any further progress. At the same time the feet and legs of the second child kept descending into the passage in spite of all my exertions to keep them back, until the first one was delivered. The woman had now been in labour nearly half a day, and from the severity of the pains, and the debility consequent on two copious bleedings, nearly exhausted. As a last resource, I determined to attempt the delivery of both children at the same time. The feet and legs of the second child were accordingly brought down; and taking advantage of a strong pain, they were both happily delivered at the same instant.

The principle of life was extinct in both. On examining the children, the difficulty of the labour was readily explained: there was a complete and perfect union between them from the top of the sternum to the umbilicus. One placenta, and one funis, supplied them both with nourishment. Their limbs were perfect, and well shaped, and features well formed. They were both females, and weighed by estimation nearly fourteen pounds—no examination was made of

their internal conformation. The woman had a quick recovery, and was able to attend to her domestic affairs, in ten or twelve days.

*A case of Hydrencephalus. By DAVID L. RODGERS, M. D.
Resident Surgeon of the New-York Hospital.*

MR. B's child, aged five months, was born with a protuberance on the os occipitis, about the size of a nutmeg. The tumour attracted little attention at first, but gradually increasing in size, it attained, at the end of two weeks, the diameter of a hen's egg, when the child evinced marked symptoms of hydrencephalus, attended with distinct fluctuation on the top of the tumour. By a gradual and continued pressure with the hand, the fluid could be completely pressed out, and the tumour diminished to one half its size. The anterior fontanelle became more and more tense, and the child exhibited signs of compressed brain. The symptoms plainly indicated that there was water in the brain, which communicated with the tumour, and Dr. Brush, the attending physician, considered it a good case for trying the practice of drawing off the water by puncture, as offering the only chance of saving the child. He accordingly made a small opening into the tumour with a lancet, by which means the fluid was freely evacuated, and all the bad symptoms immediately ceased. The parents informed me that the child did not suffer any inconvenience, and appeared to be in good health as long as the opening allowed the fluid to flow out; but on closing it, even for a time, the child invariably became affected with convulsions. The quantity of water discharged daily, was from a half a pint to a pint, which gradually enfeebled the child, and finally destroyed it.

The autopsic examination presented the following appear-

ances. The occipital bone was found deficient at its superior transverse ridge, immediately over the usual attachment of the tentorium, and portions of the cerebrum and cerebellum protruded at this place. On cutting into this part of the brain, a cavity was laid open which communicated with the two lateral ventricles. We then followed this communication into the ventricles, which were enormously enlarged, containing at least twelve ounces of fluid, and occupying the whole cranium above the tentorium, with the parieties so thin, that it appeared to verify the doctrine of Gall, that brain was a mere convolution of membranes.

I consider this case to be an important one, inasmuch as it may lead to practical consequences, and go far to establish the feasibility of puncturing the brain, as recommended by Sir A. Cooper, in this disease. The child's general health was, in the first instance, otherwise good, and its head of the ordinary size, and continued so during the whole time : yet, notwithstanding all these favourable circumstances, the discharge continued uniform as to quantity, to the last. Dr. Mott, in the case of a child, drew off the water from the ventricle by means of a small trocar, and repeated the operation several times without any permanent relief ; for the water invariably re-accumulated, and the operation was moreover attended with violent symptoms. The child is still living, and the quantity of water appears to be as great as at the time of the first operation.

I conceive it unnecessary to enter on the discussion of the propriety of tapping the head in cases of this nature, a simple statement of facts will enable every surgeon to judge what he has to expect from this operation.

A case of Hernia of the Liver. By DAVID L. RODGERS, M. D.
Resident Surgeon of the New-York Hospital.

A child of Mr. A. of Greenwich-street, was born with a tumour situated in the umbilical region, which was taken for a protrusion of intestine. The child was in other respects healthy. The tumour at the time of birth, was about two inches in diameter, and six in circumference, with the umbilical cord passing out at its apex. The covering to the sac soon after birth, became dark, insensible, and threatened mortification, the external condensed cellular substance was *in part* thrown off, and the smell from the tumour so great, that it was extremely disagreeable for any one to approach the patient. At length symptoms of obstructed liver came on, such as bilious vomiting and yellowness of the skin, and the child died in convulsions three weeks after birth.

On examining the tumour after death, it was observed to be covered with a very thin coat of peritoneum, the ordinary integuments of the abdomen being deficient over the tumour. On opening the sac, a considerable quantity of a bilious fluid flowed out, and to the surprize of every one present, the right lobe of the liver was seen protruding, making the contents of the sac, and firmly adhering and strictured at its base. By pursuing the dissection still farther, we found the abdominal muscles entirely wanting at this part.—The other viscera of the abdomen were perfectly healthy.—The child evidently died of strangulated liver.

History of two remarkable Cases of Mania (from the Memoirs of the Royal Academy of Marseilles—Vol. VIII). With observations by FELIX PASCALIS, M. D., Foreign Associate of the Royal Medical Society of Marseilles, &c.

These singular cases were reported to the Academy of Sciences of Marseilles; by J. B. Lautard, M. D., and Phy-

sician of the Lunatic Institution of that city. The remote cause of one of them can evidently be traced to considerable organic imperfections; both of them equally interest moral observers. To those who are possessed with a sound understanding, they are calculated to impart a proper diffidence respecting an undue excess of pride, or a vain conviction of personal excellence; a failing which so often underrates highly cultivated talents. We are all more or less liable to insanity, or to mental aberrations from incidental morbid causes. It is true, also, that habitual and intense thinking in contemplative minds, may often induce some degree of folly, and illusory impressions. Be it as it may, common minds, however little gifted by nature, and improved by art, and even from obscure or humble stations, daily furnish us with a considerable number of victims of mania. Unruly passions of pleasure, or aspiring ambition, can at last subvert whatever degree of sense or reason they were endowed with. To such a class belonged the following maniacs.

Louis Riny, a foundling of the Hotel-Dieu at Lyons, had been trained to chaunting church music, for which he had a remarkably fine and stentorian voice. He was a shoemaker by trade; but he would frequently lay his work aside, and, in a formidable tone, chaunt some hymn or motet, till his neighbours complained of him as an absolute nuisance. This led him to conceive, that he had become an object of jealousy and envy for his inimitable talents and musical qualifications. He resolved upon the plan of immuring himself in a cellar during night, after working hard all day, and there to give full scope to his study of sacred music. Eating very little by day, and sleeping less at night, he fell sick, and at length became a maniac, who thought that he existed in a double state, having two distinct and separate bodies, one where he actually was, and the other in any place that he had ordered him to stay. The one was the chorister,

and the other the shoemaker; the latter had always something to do in the way of his trade, and the former never spoke but in singing.

Riny was about forty years of age when sent to the Asylum. For three days he never answered any question whatsoever. At length he condescended to explain to his physician, how he possessed two bodies and two souls, or in other words, two entities; that the person he was now addressing was absent, and therefore he could not be answered. To prove his assertion, he called for *Riny* the workman in full chaunt, and then showed himself as a shoemaker. After this plain demonstration, he charged the incredulous doctor with being himself either deaf or crazy.

Malebranche or *Blaize Pascal*, who theorized upon two equal divisions of *self* or *identity*, could not have given a more conclusive argument.

The *Sosias* of Moliere, meeting with one who is perfectly similar to him, begins to suspect that he may possibly have met another self; but *Riny* entertained no doubt upon the subject. The two *ones* were in every respect well behaved, except that *Riny* the chorister would sometimes disturb and waken all the patients in the hospital with his tremendous voice.

The physician who undertook the cure of *Riny*, found that medicine only impaired his health, and that nothing could shake his belief in a double existence. The reasoning employed to dispel his illusory ideas only exasperated him, and made him accuse the doctor of insincerity. He was therefore left undisturbed; he ate, drank, and slept in proportion to the error of his double existence, and grew fat and hearty.

At length, while executing a chaunt with great vociferation, he received, from an annoyed keeper, a violent blow on the face with a broomstick; it produced an alarming hæmorrhage. After the bleeding was stopped, he fell into a

deep sleep ; a few days after, the wound closed, and he had perfectly recovered his reason. Thus the sweep of a broom put an end to the extravagant metaphysics of the double Riny. We leave it to the reader to determine, how many schools of abstract and visionary sophistry might have been benefited by the like operation.

Antony Cubisol, the second case, was thirty years of age. He was a carpenter and excelled in his trade. With a broad-axe, he could polish a plank, and with his eyes shut, divide a veneer. He was never known to have wounded himself, and his dexterity, skill, and modesty, were so much praised, that in the midst of congratulatory demonstrations of admiration he became insane. He suddenly thought that he had lost his body, that his limbs had melted away, and that nothing remained of him but a subtile breath, impossible to be fixed or detained. He abandoned his tools, they were too material for his spirituality ; he took no food, and disdaining bodily wants, ranked himself among purer intelligences. In this state he was sent to the Asylum. The physician placed him near Riny, hoping, that by the rule of *Contraria contrariis*, these two antipodes of mania, would benefit each other. TISSOT relates that a certain Dutch chymist fancied that his body would liquify, if exposed to the rays of the sun, or the heat of his furnace, and in this condition of dread and terror threw himself into a well. *Cubisol* was rather more refined in his madness ; he thought that the liquifaction of his corporeal substance had already taken place, although the power that caused it was undefinable ; and he classed himself with those essences or spirits, which, according to *Emanuel Swedenberg*, incessantly correspond with things on earth, in a mystic intercourse.

This extraordinary maniac never spoke ; he would move his lips without producing sound. He however betrayed his corporeal nature by his fears, at the threat of punishment.—

He was ever on the run, and if confined in his room, would move circularly with the greatest rapidity.

After persevering in this continued motion for a long while, he would rest for a time on one leg; but as if such a position were too material, he would raise on the great toe or ball of the foot, with his arms extended, and his head thrown back. When in this strange attitude, the physician, with a large needle, often wounded the limb which supported him; it excited no indication of pain. One might justly say, that in his ecstasy, he had lost soul and body, as the Fakirs of India, or the Santones of Egypt.

Cubisol lived fifty-seven days without food or drink. A bowl of broth was once forced down his throat, which he rejected immediately after. But when meals were served and distributed, he would run among the full bowls and dishes, inhaling the steam, and taking this respirable food with incredible eagerness. Yet, astonishing to relate, he was but little emaciated, and being perfectly free from secretion or excretion, was remarkably clean, and cold as marble, even after his ecstasies in the scorching sun of August; his lips and tongue were as red as scarlet. He lost his hair, eyelashes and eyebrows, and the nails of his feet and hands; his ears, chin, and tip of his nose, turned white, as it sometimes happens to natives of frozen regions. No arterial pulsation was perceptible throughout his frame, and some parts were entirely withered—*totumque evanescerat membrum virile*. He never slept—his respiration was, however, equal and regular, except during his moments of contemplation, when no motion of the breast was apparent. His countenance was always inexpressive, and his attention abstracted from every object around: he lost the five toes of the foot on which he used to rest, and would have lost the others, had he lived longer: he was once so deeply absorbed in reverie, that he fell on the pavement, bled a little from the nose, and shortly after expired.

The autopsic examination of *Cubisol*, presented very uncommon mal-conformations and morbid alterations in the head. His name and memory are not therefore to be left under the exclusive stain of an excessive vanity for his skill as a mechanic. His skull indeed was so unusually thick, that it took much time to remove the calvaria; and then the two lobes of the brain were seen to be of different colours; the one of orange yellow, and the other of a beet red. The falx, however, which separated them, had not been tinged by either. The operator having divided transversely, with a view to ascertain how deep the colours might have penetrated, found that of the first hemisphere, the whole substance, (the cortical excepted) could run off like cream or jelly, and that the red mass was so compact as to resist the edge of a knife. It is thus that there appeared to have been an effusion of blood into one lobe, and of serum into the other. The tentorium was the demarcation line of these strange colours.

It is proper to observe on this subject, that Morgagni, and the Memoirs of the French Academy, have recorded several instances of similar morbid alterations in the brains of maniacs and of melancholic persons. The skull of *Cubisol* was also found of one piece of bone, as it were, having no suture whatever, in which rare mal-conformation he might be said to be similar to the celebrated Cardinal Ximenés, who had been of a deep melancholic turn. The abdominal viscera were much shrunken, the intestines dry and white, interspersed with a grey paste, soapy as clay and elastic as cork. The relation of all these appearances can only be necessary to physicians, and not to the generality of readers, as some of them may be accounted for, by the patient's rigid fast of fifty-seven days. But the simple case will serve to show, that minds, ingenious but feeble, may be led to self-destruction by vanity and pride: how much more then, may not the agency of these causes endanger scientific and speculative minds, habituated to the intenser operations of the intellect,

and in self-complacent and abstruse exertions, so often verging to the extremes where wit and folly meet!

Pinel informs us that subsequently to the revolution which brought on the execution of Louis the XVI. he had no less than four maniacal *kings of France* under his care in the Hospital of Bicetre. It has also been clearly ascertained, that the greater number of victims of mental derangement are prepossessed with the idea of superior attributes: and many conquerors of old, and religious sectaries, who disturbed the peace of the world by asserting that they were privileged beings, prophets or divine messengers, are now accounted nothing more than illustrious madmen.

The errors of imagination which flatter the passions of vanity and pride, are more akin to the generality of minds, than those subjects of deep and intense study, which are the most severe upon the understanding. Mania often moulds the mind anew to a series of impressions totally different from those entertained before derangement; for, prejudices and all evil dispositions, gain through the medium of insanity a strongly marked predominance over the acquirements of education, and the force of previous examples. In fine, the most productive causes of mania, viz. vice, intemperance, unexpected prosperity, or sudden reverses, terror, the thirst of revenge, excessive love, or hatred, in short, every affection of the mind which presupposes the agency of weakness, pride, flattery, or adulation, operate equally and extensively wherever they win their way; whether it be among the high or the low, the wealthy or the poor, the old and experienced, or the young and credulous, the wise, conspicuous for talent, or the simple, confined to mediocrity. Hence, Dean Swift, calculating correctly from his many disappointments and mortifications, where his expectations had been the most sanguine, predicted that insanity would be the consequence, and the prediction was verified. Enlightened men who know how often febrile action transfers itself to the weaker parts

of the system, can easily comprehend that it may attack the sensorium, if enfeebled by the stimulus of vanity; thus it will affect their mental perception, and induce confirmed mania.

INTELLIGENCE.

Dissection of the Siren Lacertina from Georgia.

THIS reptile inhabits the muddy ditches and rice-swamps of the southern states. It differs from the serpents that have no legs, and from the lizards which have four legs, by having only two legs; and these situated on the thorax.

It was stated early after the discovery of this animal, that it possessed a most curious organization; among other peculiarities, a double set of respiratory organs; gills on the outside of the body, and lungs in the inside, were ascribed to it. This opinion, was adopted by anatomists and zoologists generally. The late Professor B. S. Barton, in 1818, wrote a memoir concerning it, which he addressed to Professor J. G. Schneider of Saxony. And in the Chevalier Cuvier's last edition of his Animal Kingdom, the Siren is placed in the *Batrachian* order, forming a genus by itself, and possessing both gills and lungs during the whole of its life.

Nevertheless, it has been asserted by high authorities, that great mistakes have been committed by those who have attempted a description of this singular creature. These are comprehended under two heads: 1st. The opponents contend that the Siren, is a larva, and not a complete animal: and 2dly. that when the larva is transformed to the perfect animal, the branches or gills disappear, as in the case of the larva of frogs and salamanders, and the lungs come into action.

For the purpose of bringing the dispute to a close, Professor Mitchill was induced, after perusing the elaborate publication of Dr. Mauro Rusconi of Pavia, on the anatomy of the Water Salamander, 1817 ; and of that gentleman, and of Professor Peter Configliacchi, on the *Proteus Anguinus* of Carniola, 1819 ; to procure for dissection the Siren of the southern states. In this he succeeded, by the aid of his friend J. Bond Read, M. D. President of the Georgia Medical Society.

An individual of the species was made the subject of a strict and careful examination, with the assistance of William Anderson, Esq. of the college of surgeons, Edinburgh, and author of the work lately published on Surgical Anatomy, whose skill in anatomical researches is so well known.

The following is from the report of that gentleman, and after a fair and full inquiry made into the matter, in May, 1822. It is in unison, with the opinion entertained by Dr. Garden, that the Siren is a perfect animal, which was countenanced by the researches of Dr. Solander among the specimens in the British Museum ; and further confirmed by the judgment of Linnæus, and the dissections of John Hunter.

“ The specimen of the *Siren Lacertina*, which I have examined, is eleven inches in length. In it, the lungs proceed from the bifurcation of the trachea at about a quarter of an inch from the commencement, continue down the back part of the abdominal cavity, and terminate within an inch of the extremity of the intestinal canal. From being injected with quicksilver, their cellular or honey-comb structure is elucidated, from which it is seen that they are not simple tubes, as stated in the memoir of Dr. Barton ; neither do they end in a bag at their lower extremities ; but gradually diminishing in size, from the middle of their length, terminate each in a point at the lower part of the abdomen.

The alimentary canal takes almost a straight direction. The intestines do not “ pass back, making many turns,” as

mentioned in the paper of Mr. John Hunter, vol. 56 of the *Philosophical Transactions*, but soon end at the anus, and do not exceed in their whole length, in the specimen before me, four inches. There is, however, a most extraordinary tubular structure situated at the back part of the abdomen on each side the spine, which has not been described or hinted at, by any who have written upon the anatomy of this animal. It extends throughout the whole length of that cavity taking a course very regularly tortuous or zigzag. It is a simple tube, but in this way inflected, even from its commencement, just behind the heart, where it is smallest; until its termination at the lower part of the abdomen which is its greater diameter; at which situation on each side it ends in a membranous sac or bag, which perhaps may be considered the blind extremities of the canal; for when injected, not a particle of mercury made its escape. I am at a loss at present, what office to ascribe to this structure, but trust we shall be able to give something new upon the mechanism of this singular animal, when we shall have examined, through the aid of a microscope, some other specimens of the *Siren Lacertina*."

I am, Sir, with much respect,

WILLIAM ANDERSON.

Samuel L. Mitchill, M. D. L. L. D.

The specimen of the *Proteus Anguinus*, in Dr. Mitchill's Museum, with the description of the distinguished Dr. Schreiber of Vienna, is an important piece of evidence in this investigation; and Mr. John Ellis's paper in the *London Philosophical Transactions*, is also an important document, published as long ago as 1766.

Measures have been taken to procure other specimens of the *Siren*; that the investigation so happily begun, may be terminated to the satisfaction of the scientific world.

Prussic Acid.

It is not one of the least objections to the use of this active medicine, that it is so very liable to be decomposed by the action of light, heat, and atmospheric air; and thus by continually varying in purity, we give in some instances a medicine capable of producing the most deleterious effects, in a quantity that in other cases, owing to deterioration, shall be absolutely inert. In order to remove this impediment, a number of physicians and other scientific gentlemen of Florence, instituted an inquiry into the powers of the *oil of the Prunus lauro-cerasus*. From a number of interesting experiments made with this article on rabbits, these gentlemen conclude that it produces the same effects, as the aqueous preparation of the acid; that it is uniform in its activity, not liable to be injured by keeping, nor by exposure to heat, light, or air; and may be advantageously employed in medicine. The best vehicles for its exhibition, are olive, or almond oil, and the mucilage of gum Arabic, in the proportion of twelve drops of the remedy to an ounce of the vehicle; of which the patient may commence with a scruple, to be gradually increased according to circumstances.

The Helminthocorton a Remedy for Cancer.

WE are indebted, it seems, for this *esquisse* of Mr. Farr's, to no less a personage than Napoleon Buonaparte. The fucus *helminthocorton*, which grows abundantly on the shores of Corsica, and has thence by the French been called *Mousse de Corse*, is frequently used on the Continent as a vermifuge. Buonaparte, whose mind was alive and attentive to all that passed within the sphere of his observation, had

remarked, during the exhibition of this medicine in cases of worms, that tumours were gradually dispersed; and in a conversation with Mr. O'Meara, expressed his surprise that it had never been tried for this purpose by the Profession. The remark was mentioned by Mr. O'Meara to Mr. Farr, who had previously been trying some of the British fuci in the treatment of tumours. Mr. Farr immediately sent to the Continent for a supply of the medicine; and has, he says, succeeded with it beyond his most sanguine expectations, in the reduction of scirrhus tumours. His practice, however, has as yet been too limited, we think, to justify us in being as sanguine as he is, respecting this medicine; but it is at all events worthy of a cautious trial, in cases where other medicines have failed. We say *cautious*, as its effects seem to be rather of a violent kind; for he speaks of nausea, vertigo, and a *bearing down of the rectum* accompanying its use, and as nothing but what may be usually expected.

The helminthocorton, according to Mr. Farr, is best given in infusion or decoction, beginning in the proportion of half an ounce to a pint of boiling water — a wineglassful to be taken thrice a day; and increasing or diminishing the dose according to the effects produced. In some cases where the bowels are not sufficiently acted upon, he combines it with rhubarb, or some other aperient. When the system is under the full effects of the medicine, the *fæces* are peculiarly characterized by green specks, by large quantities of slime and mucus, and by white substances exactly resembling dead ascarides, which Mr. Farr very properly thinks to be coagulable lymph. It is, indeed, upon the effects which it produces on the lymphatic or absorbent system, that he founds his theory of the operation of the medicine; though, as he seems half inclined to agree with Carmichael, that cancer, like worms and hydatids, has an independent vital existence; or, to speak plainly, that cancer is an animal, we think he might more plausibly have theorized on the vermifuge pro-

perties of the medicine. He has given minute details of one or two cases, which will be read with interest by those who wish to give this fucus a trial. We are sorry we cannot spare room for more particulars. We refer our readers to the work itself.

Lon. Med. Repos.

Means of Resuscitation.

TO THE EDITOR.

SIR,—As circumstances have occurred to delay the insertion of some remarks on resuscitation, in your valuable miscellany, till the Number for February, allow me to state, that their object is to induce Practitioners, (when called to attempt the renewal of functions suspended by suffocation, and other causes,) to employ, in addition to the means generally recommended, inflation of the lungs with oxygen gas, either pure, or diluted with atmospheric air; and the well-known powerful agency of galvanism. These means have been suggested, but I believe employed in *very few instances*. It appears to me that the *causes* which have hitherto prevented the general use of these means, can be only the difficulty of obtaining oxygen gas with sufficient promptitude; the want of instruments by which the inflation of that gas may be, in general, readily and effectually performed; and the want of a galvanic apparatus, portable, and, at the same time, of easy application. With the *co-operation* of a gentleman well known to, and highly esteemed by, all who prosecute scientific researches, the above have, I think, been accomplished. To make the apparatus complete in itself, so far as appears to be at present practicable, I have selected instruments recommended by others, and made such alterations and rejections as seem to be sanctioned by an attentive consideration

of the nature and causes of asphyxia, and of the circumstances required to adapt the means to the proposed end, viz. the renewal of suspended functions, less accurately called restoration of life. It does not appear to me that the principle on which inflation of the lungs should be attempted is generally understood, especially by intelligent persons *not of the Medical Profession*, who have in many instances assisted, and, in not a few, succeeded in endeavours to prolong the life of a person, who must otherwise, we may presume, have been lost to society. But I postpone other remarks for the present, merely adding, that it is my wish to publish an Essay on Resuscitation; and that I may be enabled to produce such an one as may be acceptable to the Profession, I avail myself of this opportunity to request the communication of cases, both successful and unsuccessful, with an account of the means employed, and of the appearance on examination, in cases of failure, that I may be furnished with *well-authenticated facts*; for I cannot conceive that the cases submitted to Humane Societies are recorded with *sufficient accuracy* to promote the improvement of this interesting and important branch of medicine.

I am, Sir, yours respectfully,

T. J. ARMIGER.

Eastern Dispensary, Dec. 10, 1821.

Ligatures on the Extremities recommended in certain Diseases.

DR. FRANZ, of Lissa, in Bohemia, details the case of a young woman, of seventeen years of age, who complained of a difficult, interrupted, and hurried respiration; accompanied also with pain, shooting through the region of the heart, which prevented the possibility of a profound or lengthened

inspiration. The countenance was very florid, and slightly tumefied; the pulse small, hard, and contracted; the artery felt stretched as a cord. Dr. F. prescribed tonics, with musk, and other antispasmodics; and the warm bath, with antispasmodic enemata, without any advantage. He afterwards had recourse to the application of ligatures upon the extremities, as they are recommended by J. P. Frank, in his chapter "*de spirande difficultate*," in nervous habits. "*Ligutura supra cubitum et genu injecta mieabile hinc inde levamen attulit.*" They were accordingly applied above both knees, and above the left elbow. The relief produced was both instantaneous and complete. The respiration and the pulse became freer, softer, and more tranquil; the flush in the countenance disappeared, and she was enabled to leave her bed. The ligatures were applied during three hours, when the pain and swelling which they occasioned required their removal. As soon as they were relaxed (especially the one on the left arm,) the spasms of the chest returned. They were again applied, and the relief was as immediate as before. Those upon the lower extremities were afterwards removed, and that on the arm retained for some time, and relaxed at intervals. There was no return of the disease, and the patient rapidly recovered.

Dr. Franz found the same method successful in a similar case which subsequently came under his care; no other treatment was employed at the same time in either instance.

The symptoms detailed by Dr. Frantz would appear, in our opinion, to have required blood-letting. Did not the ligatures, however, operate in a somewhat similar manner,—namely, by intercepting the return of blood to the heart, and thereby allowing that organ to overcome the distention of its cavities and neighbouring large vessels, which had previously existed, and which most probably had been the cause of the symptoms described? The relaxation of all the ligatures, if not gradually performed, would occasion, by the

sudden and overwhelming flow of blood to the right side of the heart, a recurrence of the symptoms, which were merely the efforts of that organ to relieve itself, accompanied with those signs of irritation and spasm which are generally the concomitants of action induced in any part beyond its natural functions.

We would recommend the same method, as deserving of a trial in similar affections; at least in such cases wherein a pathology such as that alluded to may be presumed to exist.



Medicinal Use of Iodine.

Dr. Valentine, of Nantz, in a letter to Dr. Pascalis, states that during his late visit to Geneva, Dr. Coindet exhibited to him many testimonies of the efficacy of Iodine in goitre; since that time, however, he has met with instances where its use has been attended with unfavourable effects. He declares it to be a *specific* in scrophula; and other physicians of Geneva speak very favourably not only of its use in this disease, but in every kind of lymphatic engorgement. Dr. Coindet has also employed Iodine in the form of ointment as a topical application to goitre and scrophulous tumours with the best effects.

The following is his recipe for making the ointment:

R Iodine—gr. xxxvj.

Hogslard—3j. m.

Of this, a quantity about the size of a nutmeg is to be rubbed on the goitre or scrophulous tumour night and morning. If there be much inflammation of the part, the ointment must be discontinued until it shall have subsided.

Academic Notices.

At a late sitting of the Society of Medicine of Paris, DR. FELIX PASCALIS of this city, was appointed *Special Foreign Correspondent*.

University of the State of New-York.

At a meeting of the Regents of the University, held at Albany, on the 11th of January last, the following promotions and appointments were made in the College of Physicians and Surgeons in the city of New-York :

WRIGHT POST, M. D. President, in the room of SAMUEL BARD, M. D. deceased.

DAVID HOSACK, M. D. Vice-President, in the room of WRIGHT POST, M. D.

JACOB DYCKMAN, M. D. and JOHN B. BECK, M. D. Trustees.

At a subsequent meeting of the Regents, the following appointment was made in the College of Physicians and Surgeons of the Western District :

JAMES M-NAUGHTON, M. D. Professor of Anatomy and Physiology, in the room of DELOS WHITE, M. D. resigned.

Annual Commencement of the College of Physicians and Surgeons, City of New-York.

Agreeably to an ordinance of the Honourable the Regents of the University of New-York, the public commencement was held in the hall of the College of Physicians and Surgeons, in the city of New-York, on the second day of April, 1822, when the following gentlemen were admitted to the degree of Doctor of Medicine, after having complied with the statutes of the University, and defended their respective inaugural dissertations.

NOVA-SCOTIA.—Ebenezer Fitz Harding, *on the Management of the Placenta.*

CONNECTICUT.—Rufus Blakeman, *on Melancholia*—Stephen Middlebrook, *on Puerperal Fever*—Henry Palmer, *on Dropsy.*

NEW-YORK.—Samuel Barrowe, jun. *on Iritis*—John Cadle, A. M. *on Mortification*—John P. Emmet, *on the Chemistry of Animated Matter*—Peter Forrester, A. B. *de Morbis Artus Genu*—Nathan S. Jarvis, *on Angina Pectoris*—Ferdinand Ludlow, *on Hydrophobia*—Robert P. Macomber, *on Menstruation*—John W. Post, *on Diseases of the Hip Joint*—David L. Rogers, *on Carotid Aneurism*—Walter S. Smith, *on Emetics in Hysteria and Epilepsy*—Lewis Traver, *on Hepatitis*—Spencer Wood, A. B. *on Hereditary Diseases*—Andrew Van Dyck, *on the Topography and Diseases of Kinderhook.*

NEW-JERSEY.—John S. Condit, A. B. *on the Lachrymal Gland*—Lewis D. Ford, *on Dysentery*—Thomas L. Smith, *on Cataract.*

VIRGINIA.—Horace Ames, *on the Phelgmasiæ.*

MORTH-CAROLINA.—Bennet Boddie, *on the Functions of the Skin*—Samuel Grier, *on Hepatitis*—William H. Hunter, *on Dyspepsia.*

SOUTH-CAROLINA.—William E. Ellerbe, *on Urethritis*—Aaron Lopez, *on the Importance of Nosology*—David I. Means, *on the Momordica Elaterium*—Edmund C. Park, *on Catarrhus Senilis*—William Porcher, A. B. *on the indigenous Gentians of the United States.*

GEORGIA.—Fitzgerald Bird, *on the Sanguinaria Canadensis.*



NOW IN THE PRESS.

The Physicians Pocket Synopsis, affording a concise view of the Symptoms and Treatment of Medical and Surgical Diseases incident to the human frame; compiled from the best authorities, with references to the most approved modern authors—Together with the Properties and Doses of the Simples and Compounds of the National Pharmacopeia of the United States—Alphabetically arranged. By J. S. Bartlett, M. D. of the Royal College of Surgeons, London, Fellow of the Massachusetts Medical Society, &c. Boston, printed by Monroe & Francis, 1822.

APRIL, 1821.

PLACES.	THERMOMETER.									Hottest day.	Coldest day.	WINDS				
	Highest Deg.			Lowest Deg.			Mean Temperature.					N.	N.W.	N.E.	E.	S.E.
	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.			days	days	days	days	days
Mackinac,	42	48	42	11	12	18	26.83	34.33	29.10	Mon. 30	Mon. 2.	5	3	3	6	
Plattsburgh,	58	64	56	20	31	27	33.76	45.93	40.70	Mon. 30.	Tues. 3.	6	1		2	
St. Peters,	57	78	61	18	28	20	35.86	48.26	37.76	Fri. 27.	Mon. 9.	5	7	2	2	2
Sackett's Harbour,	50	62	62	20	24	30	39.10	43.80	42.69	Sat. 28	Tues. 3.	3	3	4	1	5
Portland,	45	64	53	20	34	30	35.73	48.76	37.66	Sun. 29	" 3.		14	1	1	
Fort Niagara,	44	64	44	24	29	27	34.76	42.33	36.20	Mon. 23.	" 3.	4	7	6	2	1
Portsmouth, N. H.	54	68	60	24	35	30	37.20	46.56	42.33	Mon. 30.	Wed. 4.	1	9		4	1
Detroit,	60	72	64	28	32	28	38.46	48.16	43.56	Sun. 29	Tues. 17.	3		1	7	3
Watervliet, N. Y.	61	71	64	21	35	29	37.56	52.46	44.40	Mon. 30.	" 3.	2	8	1		
Prairie du Chien,	48	72	66	20	32	29	33.73	48.56	38.83	Fri. 27.	Mon. 9.	3	9	3		8
Boston,	47	71	59	22	34	30	38.16	49.93	40.53	Sun. 29.	Tues. 3.		7	7	1	1
Council Bluffs,	58	78	60	19	37	25	38.83	53.96	42.40	Fri. 27	" 17.	10	6	3		7
New-Port, R. I.	55	64	55	30	35	28	42.70	49.56	38.56	Sun. 29.	" 3.	1	7	3		4
Pittsburgh arsenal,	58	58	59	40	45	45	48.50	49.60	50.66	Fri. 27.	Sun. 1.		6	3	4	7
Fort Mifflin, Pa.	57	76	56	28	36	30	44.26	57.13	45.40	Tues. 24.	Tues. 3.		5	5	1	1
Fort McHenry, Md.	60	80	68	24	42	34	42.26	59.13	49.26	Mon. 30.	" 3.		9	5	1	7
Fort Severn, Md.	52	74	60	26	40	34	43.93	56.66	49.26	Wed. 25.	" 3.	3	7	2	3	2
Fort Washington, Md.	66	74	69	36	48	40	54.43	62.20	57.96	Mon. 30.	" 3.	7	11		1	
Norfolk, Va.	68	70	69	42	56	44	59.93	63.83	61.06	Sun. 8.	Wed. 4.		5	2		10
Fort Johnson, N. C.	62	74	68	52	48	40	61.20	65.33	63.93	Mon. 16.	Wed. 18.	5		2	6	3
Camp Ripley,	69	83	66	41	72	62	58.90	71.42	65.00	Mon. 30.	Mon. 23		7	4		16
Montpelier, Alabama,	65	80	76	40	58	46	60.90	71.86	64.60	Wed. 25.	Wed. 18.	2	6	4		3
Fort Scott, Geo.	70	82	71	44	56	52	63.50	74.90	65.93	Mon. 30.	" 18.		3		1	6
Fernandina,	76	80	74	45	56	50	63.20	70.63	65.83	Fri. 13.	Tues. 3.		2	13	1	11
Fort Gadsden,	74	78	70	46	58	53	62.46	74.06	64.20	Fri. 13.	Wed. 18.	1	3	4	1	1
Fort St. Philip, La.	69	76	73	48	60	58	64.40	70.46	67.20	Mon. 16.	" 18.		1	8	2	5

of natural science.

In the year 1805, Mr. A. Cooper made the first attempt to cure an aneurism of this artery, by securing it in a ligature ; and although it proved fatal, yet, from the length of time

APRIL, 1821.

day.	Coldest day.	WINDS								Prevailing.	WEATHER		
		N.	N.W.	N.E.	E.	S.E.	S.	S.W.	W.		Fair.	Clou.	Rai
		days	days	days	days	days	days	days	days		days	days.	day
30	Mon. 2.	5	3	3	6				13	W.	23		1
30.	Tues. 3.	6	1		2		14	3	4	S.	11	12	3
27.	Mon. 9.	5	7	2	2	2	6	4	2	N. W.	16	9	2
28	Tues. 3.	3	3	4	1	5	3	5	6	W.	15	10	2
29	" 3.		14	1	1		2	11	1	N. W.	16	10	3
23.	" 3.	4	7	6	2	1	4	4	2	N. W.	7	18	4
30.	Wed. 4.	1	9		4	1	10	2	3	S.	20	8	1
29	Tues. 17.	3		1	7	3	8	3	5	S	18	9	
30.	" 3.	2	8	1			14	1	4	S.	17	8	
27.	Mon. 9.	3	9	3		8	1	2	4	N. W.	18	7	3
29.	Tues. 3.		7	7	1	1	1	12	1	S. W.	18	9	1
27	" 17.	10	6	3		7		3	1	N.	14	9	3
29.	" 3.	1	7	3		4	2	11	2	S. W.	18	7	4
27.	Sun. 1.		6	3	4	7	4	4	2	S. E.	23		7
24.	Tues. 3.		5	5	1	1		17	1	S. W.	16	9	4
30.	" 3.		9	5	1	7	3	5		N. W.	23	2	5
25.	" 3.	3	7	2	3	2	11		2	S.	13	8	7
30.	" 3.	7	11		1		7	2	2	N. W.	15	9	6
8.	Wed. 4.		5	2		10		13		S. W.	16	7	6
16.	Wed. 18.	5		2	6	3	11	1	2	S.	14	8	8
30.	Mon. 23		7	4		16	1	1	1	S. E.	13	5	12
25.	Wed. 18.	2	6	4		3	6	7	2	S. W.	16	4	10
30.	" 18.		3		1	6	8	7	5	S.	7	10	13
13.	Tues. 3.		2	13	1	11		2	1	N. E.	13	5	12
13.	Wed. 18.	1	3	4	1	1	4	9	7	S. W.	17	7	6
16.	" 18.		1	8	2	5	5	9		S. W.	18	5	7

WEATHER.			Prevailing.	REMARKS.
Cloudy. days.	Rain. days.	Snow. days.		
	1	6	Fair	
2	3	4	Cloudy	
9	2	3	Fair	18th the Mississippi clear of ice
10	2	3	Fair	
10	3	1	Fair	
8	4	1	Cloudy	
8	1	1	Fair	
9		3	Fair	
8		5	Fair	
7	3	2	Fair	
9	1	2	Fair	
9	3	4	Fair	
7	4	1	Fair	
	7		Fair	
9	4	1	Fair	
2	5	1	Fair	
8	7	2	Fair	
9	6		Fair	
7	6	1	Fair	
8	8		Fair	
5	12		Fair	
4	10		Fair	
0	13		Rain	
5	12		Fair	
7	6		Fair	
5	7		Fair	

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MAY, 1821.

PLACES.	THERMOMETER.									Hottest day.	Coldest day.	WINDS.											
	Highest Deg.			Lowest Deg.			Mean Temperature.					N.	N.W.	N.E.	E.	S.E.	S.	S.W.					
	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.			days	days	days	days	days	days	days					
Mackinac,	62	64	55	34	37	32	43.	32	49.	00	43.	45	Tues. 8.	Sun. 13	6			11					
Plattsburgh,	56	72	66	42	41	40	46.	51	61.	14	54.	29	Tues. 29.	Sat. 5.	6			3	5	6			
St. Peters,	59	75	54	43	55	46	50.	32	65.	83	55.	09	Tues. 22.	Tues. 1.	2	4	3	1	6	4			
Sackett's Harbour,	52	78	70	42	48	48	53.	58	58.	83	57	12	Wed. 16	Mon. 14.	4	6	7		3	4			
Portland,	66	89	60	36	38	42	51.	84	60	61	50	06	Thu. 31	Sat. 5.	2	8	3	1	4	5			
Fort Niagara,	64	76	64	38	46	36	46.	50	57.	06	47	87	Mon. 28.	Thu. 3.	6	5	7	3		5			
Portsmouth, N. H.	66	78	68	40	40	40	50.	87	59.	03	54.	87	Thu. 31.	Sun. 6.	2	7	3	1	2	11			
Detroit,	52	78	76	42	52	48	53	03	65.	12	59	48	Sat. 12	Thu. 3.	2	1	3	8	7	7			
Watervliet, N. Y.	59	80	76	47	46	44	52.	83	70.	19	60.	09	Mon. 28.	Sat. 5.	11	2			4	11			
Prairie du Chien,	59	76	61	39	58	45	51.	03	65.	41	57.	25	Mon. 28	Tues. 1.	3	11	1	2	9	2			
Boston,	66	84	58	42	45	38	53.	20	58.	20	51.	61	Thu. 31.	Sun. 6.			9	1	4	1			
Council Bluffs,	55	82	60	53	43	45	53.	61	65.	41	56.	74	Sat. 5.	Thu. 17.	7	6	5	2	10				
New-Port, R. I.	65	75	57	41	41	41	56.	03	62.	64	52.	52	Mon. 28.	Sat. 5.	5	3	3	1	7	4			
Pittsburgh arsenal,	72	73	79	56	55	55	67.	54	67.	38	65.	67	Tues. 29.	Thu. 3.			7		10	4			
Fort Mifflin, Pa.	72	90	68	44	52	44	57.	41	71.	29	61	19	Wed. 30.	Fri. 4.		12		1	4				
Fort McHenry, Md.	72	94	82	46	56	48	56.	48	74.	90	65	61	Tues. 29.	Fri. 4.	1	7	3	4	9	4			
Fort Severn, Md.	70	89	78	46	55	48	58.	12	69.	77	61.	64	Tues. 29	Fri. 4.	2	5	6	3		14			
Fort Washington, Md.	69	90	80	50	60	56	57.	22	65.	13	62.	54	Tues. 29.	Wed. 23.	3	12	1		2	4			
Norfolk, Va.	78	82	80	59	60	59	68.	90	75.	03	70.	09	Wed. 30	Fri. 4.		2	4		8	2			
Fort Johnson, N. C.	78	85	72	62	70	68	71.	32	76.	09	73.	22	Thu. 31	Fri. 4.	9	3	3	3		7			
Camp Ripley,	77	90	84	62	82	70	73.	80	83.	43	75.	61	Mon. 7.	Sat. 26.		3			27				
Montpelier, Alabama,	71	91	89	68	81	71	72.	54	84.	64	75.	58	Tues. 8.	Sat. 19.		14	1	2	3	1			
Fort Scott, Geo.	72	87	72	73	75	70	70.	83	83.	25	71.	51	Thu. 31.	Sun. 13.				10		14			
Fernandina,	76	86	81	69	74	70	73.	90	81.	51	77	26	Fri. 25	Mon. 7.		1	1	3	22	1			
Fort Gadsden,	72	89	76	66	80	69	72.	48	82.	16	73.	87	Thu. 3.	Tues. 1.		9			1	2			
Fort St. Philip, La.	74	84	79	68	78	74	72.	70	79.	29	75.	64	Wed. 20	Fri. 4.				6		10	6		

MAY, 1821.

Hotest day.	Coldest day.	WINDS.								Prevailing.	WE	
		N.	N.W.	N.E.	E.	S.E.	S.	S.W.	W.		Fair	Clo
		days	days	days	days	days	days	days	days		days	day
45	Tues. 8.	Sun. 13.	6			11			14	W	23	3
29	Tues. 29.	Sat. 5.	6		3	3	5	6	4	N. & S.	16	4
(9	Tues. 22.	Tues. 1.	2	4	3	1	6	4	7	W	20	3
12	Wed. 16.	Mon. 14.	4	6	7		3	4	3	N.E.	20	7
66	Thu. 31.	Sat. 5.	2	8	3	1	4	5	6	N.W.	14	17
87	Mon. 28.	Thu. 3.	6	5	7	3		5	3	N.E.	14	10
87	Thu. 31.	Sun. 6.	2	7	3	1	2	11	3	S.	19	8
48	Sat. 12.	Thu. 3.	2	1	3	8	7	7	2	E.	15	10
(9	Mon. 28.	Sat. 5.	11	2			4	11	3	N. & S.	20	7
25	Mon. 28.	Tues. 1.	3	11	1	2	9	2	1	N.W.	18	9
61	Thu. 31.	Sun. 6.			9	1	4	1	16	S.W.	19	9
74	Sat. 5.	Thu. 17.	7	6	5	2	10		1	S.E.	14	1
32	Mon. 28.	Sat. 5.	5	3	3	1	7	4	8	S.W.	16	7
67	Tues. 29.	Thu. 3.			7		10	4	7	S.E.	25	
19	Wed. 30.	Fri. 4.		12	2	1	4		11	N.W.	15	9
61	Tues. 29.	Fri. 4.	1	7	3	4	9	4	1	S.E.	19	
64	Tues. 29.	Fri. 4.	2	5	6	3		14	1	S.	18	4
54	Tues. 29.	Wed. 23.	3	12	1		2	4	2	N.W.	20	3
(9	Wed. 30.	Fri. 4.		2	4		8	2	14	S.W.	19	9
22	Thu. 31.	Fri. 4.	9	3	3	3		7	1	N.	16	7
61	Mon. 7.	Sat. 26.		3			27		1	S.E.	19	3
58	Tues. 8.	Sat. 19.		14	1	2	3	1	5	N.W.	19	
51	Thu. 31.	Sun. 13.				10		14	7	S.	13	1
26	Fri. 25.	Mon. 7.		1	1	3	22	1	2	S.E.	13	9
87	Thu. 3.	Tues. 1.		9			1	2	11	S.W.	21	9
64	Wed. 20.	Fri. 4.			6		10	6	9	S.E.	21	9

WEATHER.				Prevailing.	REMARKS.
Fair days	Clou. days.	Rain. days.	Snow. days.		
23	3	4	1	Fair	8th the lake clear of ice.
16	5	10		Fair	
20	3	8		Fair	
20	7	4		Fair	
14	11	6		Fair	
14	10	7		Fair	
19	5	7		Fair	
15	10	6		Fair	
20	7	4		Fair	
18	9	4		Fair	
19	2	10		Fair	
14	9	8		Fair	
16	7	8		Fair	
25		6		Fair	
15	9	7		Fair	
19		12		Fair	
18	4	9		Fair	
20	3	8		Fair	
19	9	3		Fair	
16	7	8		Fair	
19	3	9		Fair	
19		12		Fair	
13	5	13			
13	2	16		Rain	
21	2	8		Fair	
21	2	8		Fair	

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JUNE, 1821.

PLACES.	THERMOMETER.									Hottest day.	Coldest day.	WINDS.							
	Highest Deg.			Lowest Deg.			Mean Temperature.					N.	N.W.	N.E.	E.	S.E.	S.	S.W.	
	VII.	II.	IX.	VII.	II.	IX.	VII.	II.	IX.			days	days	days	days	days	days	days	
Mackinac,	69	80	77	48	47	47	57.66	64.46	57.03	Mon. 25.	Wed. 13.	3	1	3	9		1		
Plattsburgh,	68	92	80	55	61	45	61.40	73.60	65.60	Mon. 25.	Sun. 3	2	3		3	1	14	2	
St. Peters,	80	92	80	59	82	64	68.66	82.43	71.73	Tues. 12.	Fri. 1.	3	1	4	2	3	9	5	
Sackett's Harbour,	72	89	89	58	60	58	66.66	72.16	69.50	Tues. 12.	Fri. 15.	8	4	3		2	3	6	
Portland,	62	90	67	55	53	49	60.56	73.83	61.36	Mon. 4.	Thu. 14.	4	1	3	1	5	2	14	
Fort Niagara,	74	88	68	50	60	56	62.56	73.03	64.73	Tues. 12.	Fri. 1.	2	11	6				6	
Portsmouth, N. H.	70	85	78	50	60	60	61.23	71.16	64.30	Tues. 5.	Fri. 15.		9		5	7	7		
Detroit,	82	92	84	58	74	68	69.30	78.96	73.83	Tues. 12.	Sun. 17.		2	4	3	4	13	1	
Watervliet, N. Y.	75	94	84	55	56	54	65.03	78.70	72.36	Tues. 26.	Fri. 1.	6	1	1	1	1	14	1	
Prairie du Chien,	70	90	84	55	82	65	65.13	79.86	71.43	Mon. 11.	Sat. 2.	1	6	1		13	2	4	
Boston,	69	95	65	53	52	50	65.50	75.33	61.53	Tues. 26.	Fri. 1.		3	8	2	2	1	13	
Council Bluffs,	82	93	80	57	77	62	70.20	83.43	71.33	Mon. 11.	Fri. 1.	3	4	4	2	12	3		
New-Port, R. I.	75	83	66	58	60	52	66.90	72.46	61.10	Mon. 25.	Fri. 1.	1	2	5		5	4	13	
Pittsburgh arsenal,	92	94	94	68	70	68	76.40	77.80	78.73	Sun. 17.	Sat. 30.	4		6	4	6	7	3	
Fort Mifflin, Pa.	81	92	76	72	66	52	71.90	80.73	71.13	Tues. 26.	Fri. 1.		6	4	1	10		7	
Fort McHenry, Md.	76	98	86	58	84	70	71.40	87.66	76.80	Mon. 25.	Sat. 2.	3	2	1	1	10	7	5	
Fort Severn, Md.	70	89	76	60	68	62	69.70	80.20	71.56	Sun. 24.	Sat. 2.	3	3	6		1	17		
Fort Washington, Md.	80	88	78	73	74	65	71.20	78.13	76.60	Tues. 26.	Fri. 1.	3	2		1	3	10	9	
Norfolk, Va.	80	85	84	67	68	69	75.63	81.66	77.26	Fri. 15.	Mon. 4.	1		3	3	4	6	10	
Fort Johnson, N. C.	82	84	89	67	72	70	77.43	81.86	78.30	Tues. 12.	Sun. 3.	18	1	1	1		1	2	
Camp Ripley,	79	91	82	70	74	70	75.85	87.56	79.56	Sun. 10.	Sat. 2.		6			22	2		
Montpelier, Alabama,	81	92	81	72	80	74	76.00	85.31	75.47	Sat. 30.	Mon. 4.								
Fort Scott, Geo.	81	90	85	70	73	71	76.46	85.03	76.46	Sat. 30.	Sun. 3.	2		2	3	10	8	5	
Fernandina,	86	92	90	72	79	75	78.50	85.03	81.43	Thu. 28.	Mon. 4.		2	3	2	12	2	9	
Fort Gadsden,	78	89	77	67	79	72	74.36	83.66	75.53	Sun. 24.	Fri. 8.		1	1	1	6	5	14	
Fort St. Philip, La.	82	87	83	74	78	76	78.30	82.33	79.53	Sat. 30.	Tues. 5.		1		5	8	6	8	

JUNE, 1821.

Coldest day.	WINDS.								Prevailing.	WEATHER.			
	N.	N.W.	N. E.	E.	S. E.	S.	S. W.	W.		Fair	Clou.	Rain.	Snow
	days	days	days	days	days	days	days	days		days	days	days.	days
l. 13.	3	1	3	9		1		13	W.	24	4	2	
3	2	3		3	1	14	2	5	S.	21	5	4	
1.	3	1	4	2	3	9	5	3	S.	16		14	
15.	8	4	3		2	3	6	4	N.	21	4	5	
l. 14.	4	1	3	1	5	2	14		S. W.	23	5	2	
1.	2	11	6				6	5	N. W.	18	10	2	
15.		9		5	7	7		2	N. W.	20	6	4	
l. 17.		2	4	3	4	13	1	3	S.	24		6	
1.	6	1	1	1	1	14	1	5	S.	21	4	5	
2.	1	6	1		13	2	4	3	S. E.	15	10	5	
1.		3	8	2	2	1	13	1	S. W.	22	4	4	
1.	3	4	4	2	12	3		2	S. E.	27	3		
1.	1	2	5		5	4	13		S. W.	19	8	3	
l. 30.	4		6	4	6	7	3		S.	25		5	
1.		6	4	1	10		7	2	S. E.	14	10	6	
2.	3	2	1	1	10	7	5	1	S. E.	23		7	
2.	3	3	6		1	17			S.	22	3	5	
1.	3	2		1	3	10	9	2	S.	23	1	6	
on. 4.	1		3	3	4	6	10	3	S. W.	21	7	2	
on. 3.	18	1	1	1		1	2	6	N.	16	2	12	
at. 2.		6			22	2			S. E.	6	7	17	
on. 4.													
on. 3.	2		2	3	10	8	5		S. E.	10	2	18	
on. 4.		2	3	2	12	2	9		S. E.	17	8	5	
1. 8.		1	1	1	6	5	14	2	S. W.	9	5	16	
es. 5.		1		5	8	6	8	2	S. E. & S. W.	4	12	14	

Snow.		Prevailing.	REMARKS.
days.			
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Fair		
	Rain		
	Rain		
	Fair		
	Rain		
	Rain		

Obser. of winds & wea. incom.

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